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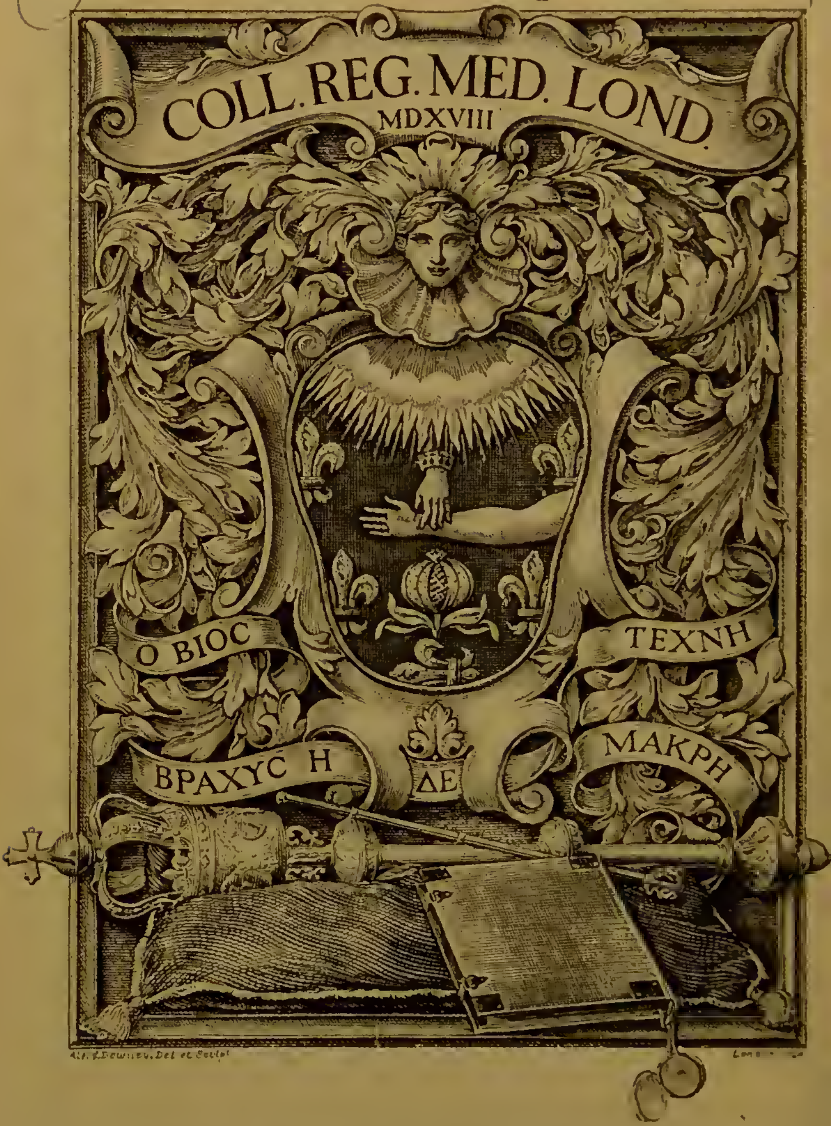

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CHEMICAL ESSAYS.

BY

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VOL. II.

FOURTH EDITION.

LONDON:
PRINTED FOR T. EVANS,
PATERNOSTER-ROW.
MDCCLXXXVII.

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ESSAY

ESSAY I.

OF THE COMPOSITION AND ANALYSIS OF GUNPOWDER.

GUNPOWDER is an artificial compoſition, conſiſting of *ſalt-petre*, *ſulphur*, and *charcoal.* The principal things to be reſpected in the making of gunpowder are,—the goodneſs of the ingredients;—the manner of mixing them;—the proportion in which they are to be combined;—and the drying of the powder after it is made.

Saltpetre, in its crude ſtate, whether it be brought from the Eaſt Indies, or made in Europe, is generally if not univerſally, mixed with a greater or leſs portion of common ſalt: now a ſmall portion of common ſalt injures the goodneſs of a large quantity of gunpowder, hence it becomes neceſſary, in making gunpowder, to uſe the very fineſt ſaltpetre.—The pureſt ſulphur, is that which is ſold in the ſhops under the name of flowers of ſulphur; but the roll ſulphur, being much cheaper than the flowers of ſulphur, and being alſo of a great degree of purity, it is the only ſort which is uſed in the manufacturing of gunpowder.—With relation to the charcoal, it has been generally believed that the coal from ſoft and light woods was better adapted to the making of gunpowder, than that from the hard and heavy ones; thus *Evelyn* ſays of the hazel that "it made one of the beſt coals uſed for gunpowder, being very fine and light, till they found alder to be more fit*." And in another place he thinks that lime-tree coal is ſtill better than that from alder †. An eminent French chemiſt has ſhewn, from actual experiment, that this opinion in favour of coal from light woods is ill founded; he affirms, that powder made from lime-tree coal, or even from the coal of the pith of elder tree, is in no reſpect preferable to that made from the coal of the hardeſt woods, ſuch as guaiacum and oak‡. This remark, if it be confirmed by future experience, may be of uſe to the makers of gunpowder; as it is not always an eaſy matter for them to procure a ſufficient quantity of the coal of ſoft wood.

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* Evelyn's Silva, by Dr. Hunter, p. 223.

† Id. p. 946.

The mixture of the materials of which gunpowder is made, ſhould be as intimate and as uniform as poſſible; for, in whatever manner the exploſion may be accounted for, it is certain that the three ingredients are neceſſary to produce it. Saltpetre and ſulphur mixed together give no exploſion; ſulphur and charcoal give no exploſion; and though ſaltpetre and charcoal, when intimately mixed, do give an exploſion, yet it is, probably, of far leſs force than what is produced from a mixture of the three ingredients. I have ſaid, *probably*; becauſe this point does not ſeem to be quite ſettled at preſent, as may appear from the following opinions, of two eminent chemiſts, each of whom appeals to experience. —— " Un mêlange de ſix onces de nitre et d' une once de charbon produit une poudre qui a *moitié moins* de force que toutes celles dans leſquelles on fait entrer du ſoufre : cette ſubſtance eſt donc abſolument eſſentielle à la compoſition de la poudre. Dans le temps que je travaillois ſur cette matiere, quelques particuliers propoſerent de faire de la poudre ſans ſoufre : ils promettoient qu' elle ſeroit plus forte, — La poudre dans laquelle on fait entrer une petite quantitè de ſoufre,

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‡ Chym. par M. Baumé. Vol. I. p. 455.

augmente de force *du double*"*.—"The principal ingredients of gunpowder, and thoſe to which it owes its force, are nitre and charcoal; for theſe two ingredients well mixed together, conſtitute gunpowder at leaſt *equal*, if not *ſuperior in ſtrength* to common gunpowder (as I found by experience), and may be ſeen in the Memoire of Count Saluce inſerted in the *Melanges de Philoſophie et de Mathematiques, de l'Academie Royale de Turin.* The ſulphur ſeems to ſerve only for the purpoſe of ſetting fire to the maſs with a leſs degree of heat"†.—If I may truſt ſome crude experiments which I have made with a com-

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[*] Chy. par M. Baumé. Vol. I. p. 461.

[†] Philoſ. Tranſ. 1779. p. 397, where the reader will find ſeveral ingenious experiments relative to the nature of gunpowder by Dr. Ingenhouſz.

a common powder Trier, I muſt accede to the opinion of M. Baumé; as I repeatedly found that equal *bulks* of common powder, and of the ſame ſort of powder, freed from its ſulphur by a gentle evaporation, differed very much both in the loudneſs and force of the exploſion; the powder which had loſt its ſulphur being inferior to the other in both particulars. It is not without reaſon, that equal *bulks* are here ſpecified, for any definite meaſure of common powder weighs more than the ſame meaſure of powder which has loſt its ſulphur; hence the reſult of experiments made with equal *weights* of theſe powders, will be different from that which is derived from the exploſion of equal *bulks:* may not this obſervation tend to reconcile the opinions before mentioned? But whether ſulphur be an abſolutely neceſſary ingredient in the compoſition of good gunpowder or not, it is certain that an accurate mixture of the ingredients is eſſentially requiſite. In order to accompliſh this accurate mixture, the ingredients are previouſly reduced into coarſe powders, and afterwards ground and pounded together, till the powder becomes exceeding fine; and when that is done the gunpowder is made. But as gunpowder, in the ſtate of an impalpable duſt, would be inconvenient in its uſe, it has been cuſtomary to reduce it into grains, by forcing it, when moiſtened with water, through ſieves of various ſizes.

The neceſſity of a complete mixture of the materials, in order to have good gunpowder, is ſenſibly felt, in the uſe of ſuch as has been dried after having been accidentally wetted. There may be the ſame weight of the powder after it has been dried, that there was before it was wetted; but its ſtrength is greatly diminiſhed on account of the mixture of the ingredients being leſs perfect. This diminution of ſtrength proceeds from the water having diſſolved a portion of the ſaltpetre (the other two ingredients not being ſoluble in water); for upon drying the powder, the diſſolved ſaltpetre will be cryſtallized in particles much larger than thoſe were, which entered into the compoſition of the gunpowder, and thus the mixture will be leſs intimate and uniform, than it was before the wetting. This wetting of gunpowder, is often occaſioned by the mere moiſture of the atmoſphere. Great complaints were made concerning the badneſs of the gunpowder uſed by the Engliſh in their engagement with the French fleet off Grenada, in July, 1779; the French having done much damage to the maſts and rigging of the Engliſh, when the Engliſh ſhot would not *reach* them. When this matter was inquired into by the Houſe of Commons, it appeared that the powder had been injured by the moiſture of the atmoſphere; it had concreted into large lumps, in the middle of which the ſaltpetre was viſible to the naked eye. If the wetting has been conſiderable, the powder is rendered wholly unfit for uſe; but if no foreign ſubſtance has been mixed with it except freſh water, it may be made into good gunpowder again, by being properly pounded and granulated. If the wetting has been occaſioned by ſalt water, and that to any conſiderable degree, the ſea ſalt, upon drying the powder, will remain mixed with it, and may ſo far vitiate its quality, that it can never be uſed again in the form of gunpowder. However, as, by ſolution in water and ſubſequent cryſtallization, the moſt valuable part of the gunpowder, namely, the ſaltpetre, may be extracted, and in its original purity, even from powder that has been wetted by ſea water, or otherwiſe ſpoiled, the ſaving a damaged powder, is a matter of national œconomy, and deſervedly attended to in the Elaboratory at Woolwich.

The proportions in which the ingredients of gunpowder are combined together, are not the ſame in different nations, nor in different works of the ſame nation, even for powder deſtined to the ſame uſe. It is difficult to obtain from the makers of gunpowder, any information upon this ſubject; their backwardneſs in this particular, ariſes not ſo much from any of them fancying themſelves poſſeſſed of the beſt poſſible proportion, as from an affectation of myſtery, common to moſt manufacturers, and an apprehenſion of diſcovering to the world, that they do not uſe ſo much ſaltpetre as they ought to do, or as their competitors in trade really do uſe. Saltpetre is not only a much dearer commodity than either ſulphur or charcoal, but it enters alſo in a much greater proportion into the compoſition of gunpowder, then both theſe materials taken together; hence, there is a great temptation to leſſen the quantity of the ſaltpetre, and to augment that of the other ingredients; and the fraud is not eaſily detected, ſince gunpowder, which will explode readily and loudly, may be made with very different quantities of ſaltpetre.

Baptiſta Porta died in the year 1515; he gives three different proportions for the making of gunpowder, according as it was required to be of different ſtrength*. I have reduced his proportions, ſo that the reader may ſee the quantities of the ſeveral ingredients, contained in 100 pounds weight of each ſort of powder.

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* Mag. Nat. L. XII. c. 3.

	Weak.	Strong.	Strongeſt.
Saltpetre	66⅔ lb.	75	80
Sulphur	16⅔	12½	10
Charcoal	16⅔	12½	10
	100	100	100

It is ſomewhat remarkable, that, in all theſe proportions, the ſulphur and charcoal are uſed in equal quantities. *Cardan* died about ſixty years after Baptiſta Porta, and in that interval, the proportions of the ingredients of gunpowder ſeem to have undergone a great change. Cardan's proportions for great, middle-ſized, and ſmall guns, are expreſſed in the following table.*

Salt-

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* Card. Oper. Vol. III. p. 379.

	Gt. Guns.	Mid. ſized.	Small.
Saltpetre	50lb.	66⅔	83⅓
Sulphur	16⅔	13⅓	8⅓
Charcoal	33⅓	20	8⅓
	100	100	100

For great and middle-ſized guns, we ſee a much greater proportion of charcoal than of ſulphur, was uſed in Cardan's time; at preſent, I believe, it is in moſt places the reverſe, or at leaſt, the charcoal no where exceeds the ſulphur. I have put down the proportions uſed at preſent in England, France, Sweden, Poland, and Italy, for the beſt kind of gunpowder.

	Eng.	Fran.	Swed.	Pol.	Italy.
Saltpetre	75	75	75	80	76½
Sulphur	15	9½	16	12	12½
Charcoal	10	15½	9	8	12½
	100*	100‖	100‡	100‡	101½†

Several experiments have been lately made in France, in order to determine the exact proportions of the ſeveral ingredients which would produce the ſtrongeſt poſſible powder; theſe proportions when reduced, as all the reſt have been, to the quantity compoſing one hundred pounds of gunpowder, are

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* Theſe are ſaid to be the proportions of government powder.—Pemb. Chem. p. 207

‖ Chem. Dict. & Baumé's Chem. Vol. I. p. 466.

‡ Mem. de Chem. Vol. II. p. 425, where it is ſaid, that two ſpecimens of powder from Holland, gave only 71lb. of ſaltpetre from 100 of powder.

† Comm. Scien. Bonon. Vol. IV. p. 113.

Saltpetre	80 lb.
Charcoal	15
Sulphur	5
	100

From hence it would appear, that in a certain weight of ſaltpetre, the powder would produce the greateſt effect, when the weight of the charcoal was to that of the ſulphur, as 3 to 1. On the other hand, experiments are produced from which it is to be concluded, that in a certain weight of ſaltpetre the beſt powder is made, when the ſulphur is to the charcoal, in the proportion of 2 to 1. From theſe different accounts, it ſeems as if the problem of determining the very beſt poſſible proportion was not yet ſolved.

In drying gun-powder, after it is reduced into grains, there are two things to be avoided, too much and too little heat. If the heat is too great, a portion of the ſulphur will be driven off, and thus the proportion of the ingredients being changed, the goodneſs of the powder, ſo far as it depends on that proportion, will be injured. In order to ſee what quantity of ſulphur might be ſeparated from gunpowder, by a degree of heat not ſufficient to explode it, I took 24 grains of the powder marked F F in the ſhops, and placing it on a piece of poliſhed copper, I heated the copper by holding it over the flame of a candle; the gunpowder ſoon ſent forth a ſtrong ſulphureous vapour; and when it had been dried ſo long that no more fume or ſmell could be diſtinguiſhed, the remainder weighed 19 grains, the loſs amounting to 5 grains. The remainder did not explode by a ſpark like gunpowder, but like a mixture of ſaltpetre and charcoal, and it really was nothing elſe, all the ſulphur having been diſſipated. Gunpowder was formerly dried by being expoſed to the heat of the ſun, and this method is ſtill in uſe in France, and in ſome other countries; afterwards a way was invented of expoſing it to a heat equal to that of boiling water; at preſent it is moſt generally in England dried in ſtoves heated by great iron pots; with any tolerable caution no danger of exploſion need be apprehended from this method. All the watery parts of the gunpowder may be evaporated by a degree of heat greatly leſs than that in which gunpowder explodes, that degree having been aſcertained, by ſome late experiments, to be about the 600th degree on Fahrenheit's ſcale, in which the heat of boiling water is fixed at 212. There is more danger of evaporating a portion of the ſulphur, in this way of drying gunpowder, than when it is dried by expoſure to the ſun.

The neceſſity of freeing gunpowder from all its moiſture, is obvious from the following experiment, which was made ſome years ago before the Royal Society. A quantity of gunpowder was taken out of a barrel, and dried with a heat equal to that in which water boils; a piece of ordnance was charged with a certain weight of this dried powder, and the diſtance to which it threw a ball was marked. The ſame piece was charged with an equal weight of the ſame kind of powder, taken out of the ſame barrel, but not dried, and it threw an equal ball only to one half the diſtance. This effect of moiſture is ſo ſenſible, that ſome officers have affirmed, that they have ſeen barrels of gunpowder which was good in the morning, but which became (by attracting, probably, the humidity of the air) good for nothing in the evening*. In order to keep the powder dry, by preventing the acceſs of the

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* — qu' il avoit vu, dans les guerres d' Italie, quelques barrels de poudre qui étoit bonne le matin, et qui ne valoit rien le ſoir. Hiſt. Nat. de l' Eſpagne, p. 82.

air, it has been propoſed to line the barrels with tin foil, or with thin ſheets of lead, after the manner in which tea-boxes are lined†.—Would it not be poſſible to preſerve powder free from moiſture, and from the loſs of a part of its ſulphur in hot climates, by keeping it in glazed earthen bottles, or in bottles made of copper or tin, well corked?

This diſpoſition to attract the humidity of the air, is different in different ſorts of powder, it is the leaſt in that which is made from the pureſt ſaltpetre; pure ſaltpetre, which has been dried as gunpowder is dried, does not become heavier by expoſure to the atmoſphere; at leaſt, its increaſe of weight is very ſmall, not amounting, as far as my experiments have informed me, to above one 72d part of its weight; I rather think that it does not acquire any increaſe of weight. But ſaltpetre mixed with ſea ſalt, attracts the humidity very ſenſibly, and hence, though there ſhould be the ſame weight of ſaline matter in a certain weight of gunpowder, yet the goodneſs of the powder may be very variable, not only from the foreign ſaline matter, be it ſea ſalt, or any other ſalt, injuring the quality of the powder as being an improper ingredient, but from its rendering the powder more liable to become humid.

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† Hiſt. Nat. de l' Eſpag.

Saltpetre being the ingredient, in which there is the greateſt room for fraud, in the compoſition of gunpowder, and on the quantity of which its ſtrength chiefly depends, the reader will excuſe the minuteneſs of the ollowing proceſs, to aſcertain the quantity of ſaltpetre contained in any ſpecimen of gunpowder.

Take any quantity of gunpowder, pound it in a glaſs mortar till all the grains are broken, lay it before a gentle fire till it be quite dry, in that ſtate weigh accurately any quantity of it, ſuppoſe 4 ounces; boil theſe four ounces in about a quart of water, the boiling need neither be violent nor long continued, for the water will readily diſſolve all the ſaltpetre, or other ſaline matter, and not a particle of either the ſulphur or the charcoal of the powder. In order to ſeparate the water containing the ſaltpetre, from the ſulphur and charcoal, pour the whole into a filter made of brown paper; the water containing the ſaltpetre will run through the paper, and muſt be carefully preſerved; the charcoal and ſulphur will remain upon the paper. But as ſome particles of ſaltpetre will ſtick both to the filtering paper and to the maſs of ſulphur and charcoal, theſe are to be repeatedly waſhed, by pouring hot water upon them, till the water in running through the filter is quite inſipid; then we may be certain, that we have all the ſaltpetre originally contained in the powder, now diſſolved in the water, and all the ſulphur and charcoal remaining a mixed maſs upon the filter. Theſe reſpective quantities may be aſcertained without much difficulty. The water containing the diſſolved ſaltpetre, muſt be evaporated by a gentle heat; the ſaltpetre cannot be evaporated by the ſame degree of heat which evaporates the water, all the ſaltpetre then contained in the gunpowder, will remain after the water is diſperſed, and being carefully collected and weighed, it will ſhew the quantity of ſaline matter contained in the powder. Dry the maſs of ſulphur and charcoal, by laying the filtering paper containing it before the fire; it ſhould be made as dry as the powder was before it was diſſolved in the water: in that ſtate weigh the ſaltpetre and charcoal; and, when the experiment has been accurately made, the weight of the ſaltpetre added to that of the mixture of ſulphur and charcoal, will juſt amount to four ounces, the weight of the powder. The quantity of ſaline matter contained in any ſpecimen of gunpowder, being thus aſcertained, its quality may be known by diſſolving it in water, and cryſtallizing it; if any part of it cryſtallizes in little cubes, it is a ſign that it contains ſea ſalt; or if any part of it, after being duly evaporated, will not cryſtallize, it is a ſign that it contains another ſort of impurity, called by ſaltpetre makers, the *mother of nitre*, which powerfully attracts the humidity of the air.

The gunpowder marked F F, was analyſed in the following manner. Twenty-four grains, by evaporating the ſulphur, were reduced to nineteen; theſe nineteen grains gave, by ſolution in water and ſubſequent filtration and cryſtallization, ſixteen grains of ſaltpetre, the charcoal, when properly dried, weighed three grains.

According to theſe proportions, 100 pounds of this kind of gunpowder conſiſted of

Saltpetre	$66\frac{2}{3}$
Sulphur	$20\frac{5}{6}$
Charcoal	$12\frac{1}{2}$
	100 lb.

I tried this gunpowder in two or three other ways, by taking larger quantities of it, but the quantity of ſaltpetre was always 66 lb. together with ſome fractional part of a pound, from 100 lb. of gunpowder. The powders marked with a ſingle and a double F, differ in the ſize of the grain, but they do not ſeem to differ, as far as I have tried them, in the quantity of the ſaltpetre they contain. From ſome ſorts of powder, I have got after the rate of 76 lb. of ſaltpetre, from 100 lb. of the gunpowder.

The method of analyſing gunpowder, by evaporating the ſulphur, is not wholly to be relied upon; I have often obſerved, that when mixtures of ſulphur and charcoal have been expoſed to evaporation, on a plate of heated copper, the remainder has weighed leſs than the charcoal which entered the compoſition, part of it having been carried off by the violent evaporation of the ſulphur: and hence the proportion of ſulphur in the above analyſis is probably too great. I am aware that this obſervation is wholly oppoſite to the concluſion of M. Baumé, who contends, that one twenty-fourth part of the weight of the ſulphur employed in any mixture of ſulphur and charcoal, adheres ſo ſtrongly to the charcoal, that it cannot be ſeparated from it without burning the charcoal.—I can only ſay, that he ſeparated the ſulphur by burning it, and I ſeparated mine by ſubliming it without ſuffering it to take fire, and this difference in the manner of making the experiment, may, perhaps, be ſufficient to account for the different reſults.—But it is unneceſſary to purſue this ſubject further, there are ſeveral things to be attended to in forming a complete analyſis of gunpowder, which any perſon tolerably well verſed in chemiſtry, would certainly take notice of, if the analyſis of any particular powder was required to be made, and which cannot in this general view be minutely deſcribed: and, indeed, it is the leſs neceſſary to enter into a detail on this ſubject, as the ſtrength of the powder is not much affected by ſmall variations in the quantities of the ſulphur and charcoal, which enter into its compoſition; and the method of aſcertaining the quantity and quality of the ſaltpetre, in any particular gunpowder, has been ſufficiently explained.

In order to judge with more certainty concerning the effect of ſea ſalt, when mixed with ſaltpetre, in attracting the humidity of the air, I made the following experiment. Five parts of pure ſaltpetre in powder, were expoſed for a month to a moiſt atmoſphere, but I did not obſerve that the ſaltpetre had gained the leaſt increaſe of weight; for the ſame length of time, and in the ſame place, I expoſed four parts of ſaltpetre mixed with one of common ſalt, and this mixture had attracted ſo much moiſture, that it was in a ſtate of fluidity.

ESSAY II.

OF COMMON SALT.

COMMON ſalt is a neutral ſalt, it has neither an acid nor an alkaline taſte, nor does it change the blue colour of vegetables into a red as acids do, nor into a green as alkalies do. It conſiſts of two things, of an *acid* peculiar to itſelf, and of the alkali which is ſeparable from the aſhes of marſh ſamphire and other maritime plants, and which has been denominated the *mineral* or *foſſile fixed alkali.* The foſſile alkali, as was obſerved of the vegetable alkali, when the manner of decompoſing ſaltpetre was explained, is more powerfully attracted by the acid of vitriol than by the acid of common ſalt; hence common ſalt is effectually decompoſed when it is diſtilled in conjunction with the acid of vitriol, for this acid expels the acid of common ſalt from its union with the mineral alkali, and unites itſelf with it in its ſtead: the acid of common ſalt being thus diſengaged from its baſis, is eaſily, by the heat, raiſed in vapour, and forced into the receiver. The acid of common ſalt, thus obtained, is very volatile, conſtantly emitting white fumes; and it is uſually called the *marine acid,* the *muriatic acid, Glauber's fuming acid* or *ſpirit of ſalt.* If the reader pours a few drops of very ſtrong acid of vitriol on a ſmall portion of dry common ſalt, he will ſee a white vapour ariſing from the ſalt; this vapour is the moſt volatile part of the acid of the ſalt, and a judgment may be formed of its pungency and volatility, from its preſently infecting the air with its ſmell to a great extent. This acid vapour is a kind of air, for it retains its elaſticity for ſome time, not being readily condenſable by cold. It has engaged the attention of Dr. Prieſtley; and with his accuſtomed ingenuity, he has made ſeveral experiments, from which we learn, that this vapour when condenſed, conſtitutes an acid which is twice as heavy as rain water, and that the ſtrongeſt acid of ſalt may be conſidered as conſiſting of one third of this elaſtic vapour, and of two thirds of water*. After the extraction of the *acid*, which is one of the conſtituent parts of common ſalt, there remains, in the veſſel uſed for the diſtillation, a compound maſs, conſiſting of the *acid of vitriol* united with the *mineral fixed alkali*, the other conſtituent part of common ſalt: this is proved by diſſolving the maſs, and cryſtallizing it, for the cryſtals are preciſely the ſame as would reſult from a direct combination of the acid of vitriol with the mineral fixed alkali. The ſalt reſulting from a decompoſition of common ſalt by means of the acid of vitriol, or from a direct combination of that acid with the mineral alkali, is the *genuine Glauber's ſalt.* The artificial compoſition of common ſalt, confirms the account we have given of its conſtituent parts from its analyſis, for if we combine the acid procured from diſtilling common ſalt and acid of vitriol together, with the mineral fixed alkali, there will reſult from their union, a ſalt, in all reſpects the ſame as common ſalt, except that it will be ſomewhat more pure than common ſalt; the ſalt thus produced is called *regenerated ſalt.*

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* Philoſ. Tranſ. 1772. p. 239.

Common ſalt is generally diſtinguiſhed by mineralogical writers into three different ſorts—*foſſile ſalt—ſpring ſalt—ſea ſalt.* In chemiſtry though we admit this diviſion, as ſerviceable in pointing out the three general ſources from which common ſalt is moſt copiouſly derived, yet we reckon all theſe ſalts, when pure, to be of the ſame ſort; they are all compoſed of the *marine acid*, and of the *mineral fixed alkali.*

Foſſile or *rock ſalt*, is found in large beds in the bowels of the earth, in every quarter of the globe; it is ſometimes cryſtallized, and as tranſparent as a diamond, and it has thence been called *ſal gemmæ*; but it is more frequently found in irregular maſſes of a red, yellow, or blue colour. There are ſeveral mines of rock ſalt near *Northwich* in *Cheſhire*; the firſt of which was diſcovered, as they were boring for coal, in the year 1670*.

The ſprings which are met with, both *above* and *below* the level of the Northwich bed of rock ſalt, are ſtrongly impregnated with ſalt. This is eaſily accounted for; the rain water in ſinking through the ground which lies over the rock ſalt, at laſt arrives at the ſalt; its further deſcent is in a great meaſure obſtructed by the ſolid body of ſalt, it reſts upon it, and in reſting upon the ſalt it diſſolves it, and thus conſtitutes a brine ſpring above the level of the bed of rock ſalt. The brine ſprings, which are found below that level, probably ariſe from the water, which has diſſolved a portion of rock ſalt, in ſinking to that depth in the earth.

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* Philoſ. Tranſ. No. 66.

I have had the curioſity to go to the bottom of ſome of the moſt famous mines in England; ſuch as the copper mine at Ecton; the coal mines at Whitehaven and Newcaſtle; the cannel coal mines in Lancaſhire; and a variety of lead mines in Derbyſhire, and other places; but I never thought my labour, in theſe ſubterraneous expeditions ſo well rewarded, as in the ſight of the rock-ſalt mines at Northwich. The ſalt mines at *Wilizka* near *Cracow* in Poland, have long been a ſubject of admiration, and every traveller has exerted his deſcriptive talents, in magnifying the ſtupendous wonders which are there to be met with. Theſe mines have been wrought above five hundred years, and from that circumſtance we may readily imagine, the great number of vaſt caverns which have been formed in conſequence of the blocks of ſalt which have been annually extracted: yet it deſerves to be remarked, that the Northwich rock ſalt mines are ſuperior to thoſe of Cracow, with reſpect to the quantity of ſalt which is annually raiſed. The ſingle pit into which I deſcended at Northwich, yielded at a medium 4000 tons of ſalt in a year; this quantity is about two thirds of that which is raiſed every year in the Poliſh mines here ſpoken of.*

A great quantity of rock ſalt is uſed at Northwich, in order to ſtrengthen their brine ſprings, and a much greater quantity is ſent coaſtways to Liverpool and other places, where it is uſed either for ſtrengthening brine ſprings or ſea water; much of this rock ſalt was formerly exported to Holland, and it is ſtill ſent to Ireland for the ſame purpoſe. Rock ſalt, and the white ſalt which is at Northwich, chiefly made from rock ſalt, is exported free from the ſalt duty; and I was informed that the quantity which is annually exported from Northwich, is ſo great, that, if it paid the duty, it would bring in to government, a ſum not much ſhort of four hundred thouſand pounds a year. "According to the beſt accounts I have been able to procure, the groſs duty on ſalt made in South Britain, amounts annually to ſeven hundred thouſand pounds.*" The duty on ſalt made at Northwich is about ſeventy thouſand pounds a year, or a tenth part of the whole duty†.

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* See Memoire ſur les Mines de Sel de Wieliczka en Pologne, in Hiſt. de l'Acad. des Scien. a Par. 1762.

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[*] Camp. Sur. of Brit. Vol. II. p. 26.

[†] Since I received this information, an additional duty of 10d. a buſhel has been laid, in 1780, on ſalt. The whole duty now amounts to

The Northwich rock ſalt is never uſed at our tables in its crude ſtate; and its application to the pickling or curing of fleſh or fiſh, or preſerving any proviſions, without its being previouſly refined into white ſalt, that is, without its being diſſolved in water, and boiled down into what is called white ſalt, is prohibited under a penalty of 40s. for every pound of rock ſalt ſo applied. The pure tranſparent maſſes, however, of rock ſalt, might, probably, be uſed by us with our food, without any ſort of danger or inconvenience; at leaſt, we know that rock ſalt is ſo uſed, without being refined, both in *Poland* and in *Spain*. In the laſt of theſe countries, at

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to 4s. 2d. the buſhel, the buſhel weighing 56lb. The makers of ſalt can afford, in moſt places, to ſell their ſalt, excluſive of duty, from 8d. to 10d. a buſhel.

at *Cordona* in the province of *Catalonia*, there is a ſolid mountain of rock ſalt, between four and five hundred feet in height, and a league in circuit; its depth below the ſurface of the earth is not known*. This prodigious mountain of ſalt, which has no mixture of other matter with it, is eſteemed ſo ſingular an appearance, that it is thought to militate very much againſt the opinion of thoſe, who would derive the origin of all the beds of rock ſalt, which are found under the ſurface of the earth, from the evaporation of ſalt water, left in ſubterraneous caverns, either at the deluge, or upon ſome more local commotions of the globe.

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* Hiſt. Nat. de l'Eſpag. p. 406. See an account of ſimilar mountains of rock ſalt in Shaw's Travels, p. 229, and in Pliny's Hiſt. Nat. l. xxxi. c. 7.

The quantity of rock ſalt which may be diſſolved in a definite quantity, ſuppoſe a pint or 16 avoirdupois ounces of water, is differently eſtimated by different authors. *Boerhaave* is of opinion, that 16 ounces of water will not diſſolve quite five ounces of rock ſalt[*]; *Spielmann* thinks that they will diſſolve $6\frac{2}{3}$ ounces[†]; *Newmann* agrees with Spielmann[‡]; *Eller* ſays, that ſeven ounces of foſſile ſalt may be diſſolved in 16 ounces of water[§]; laſtly, *Hoffmann* aſſures us, that 16 ounces of water will not diſſolve above ſix ounces of common ſalt[||]. I have tried this matter with diſtilled and with common water, and in various degrees of heat, and cannot but be of opinion, that Hoffmann's experiment approaches neareſt to the truth; I never could diſſolve *quite* ſix ounces of rock ſalt in 16 ounces of water. It is not wholly improbable, that different ſorts of rock ſalt, may differ ſomewhat with reſpect to their ſolubility in water.

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[*] Chem. Vol. I. p. 476.

[†] Inſt. Chem. p. 48.

[‡] Newmann's Chem. by Lewis, p. 256.

[§] Obſ. Phy. Chem. L. ii. Ob. xvi.

[||] Ber. Mem. 1750.

If it be admitted, that 16 ounces of water can diſſolve ſix ounces of ſalt and no more, then we may be certain, that no brine ſpring, in any part of the world, can yield ſix ounces of ſalt from a pint of the brine. For brine ſprings are, ordinarily, nothing but water in which foſſile ſalt has been diſſolved; but a pint of the ſtrongeſt brine cannot contain ſo much ſalt, as is contained in a pint of water, which has been ſaturated with ſix ounces of ſalt; for a pint of water, in which ſix ounces of ſalt have been diſſolved, is increaſed a little in bulk, it will do more than fill a pint meaſure, and the ſalt left in the ſurplus will ſhew, how much the ſalt, contained in a pint of the ſtrongeſt brine, falls ſhort of ſix ounces. Or we may conſider the matter in the following manner, which will, perhaps, be more intelligible; 16 ounces of water, impregnated with ſix ounces of ſalt, conſtitute a ſaturated brine, weighing 22 ounces; if therefore we would know how much ſalt is contained in 16 ounces of ſuch brine, by the rule of proportion we may argue, that if 22 ounces of brine contain ſix ounces of ſalt, 16 ounces of brine will contain $4\frac{4}{11}$ ounces of ſalt. Hence we may infer, that the ſtrongeſt brine ſprings will not yield much above one quarter of their weight of ſalt*.

Dr. Leigh, who firſt ſhewed the manner of refining rock ſalt, informs us, that ſome of the ſtrongeſt ſprings at Northwich, gave ſeven or eight ounces of ſalt from a quart of brine; but a quart of brine weighs conſiderably more than 32 ounces, the weight of a quart of water; ſo that the Northwich ſprings, from this account,

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* —— "Several pits at *Northwich*, and at *Barton* in *Lancaſhire*, contain no leſs than ſix ounces of ſalt upon ſixteen of brine; which is as large a proportion of ſalt as water will diſſolve." Newm. Chem. p. 212, Lewis's note. The author here is fallen, probably, into a little miſtake, by confounding a pound of water with a pound of brine; for if a pound or 16 ounces of water will only diſſolve ſix ounces of ſalt, a pound of the brine, thus formed, will only hold $4\frac{4}{11}$ ounces of ſalt.

count, do not yield a quarter of their weight. At *Middlewich* there is ſaid to be one ſalt ſpring, which is ſtronger than the reſt; this ſpring yields a *full fourth* part of ſalt*, and hence it is, probably, fully ſaturated. We have an account in Kircher's works, of ſome famous brine ſprings in *Burgundy*, from which we learn, that one hundred pounds weight of the ſtrongeſt brine, gave 25 pounds, or juſt one *fourth* of its weight of white ſalt †.

There are a great many brine ſprings in *Cheſhire*, *Worceſterſhire*, *Staffordſhire*, *Hampſhire*, and in other parts of Great Britain, ſome of which are ſufficiently rich in ſalt to be wrought with profit, others not.

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* Philoſ. Tranſ. No. 53.

† Kirch. Mun. Sub. Tom. II. Cap. XI.

From what has been before advanced the reader will readily comprehend that 16 tons of the ſtrongeſt brine conſiſt of 12 tons of water, and of 4 tons of ſalt; and that, in order to obtain theſe 4 tons of ſalt, the 12 tons of water muſt be, by ſome means or other, evaporated, ſo as to leave the ſalt in a concrete form. Suppoſe there ſhould be a brine, which in 16 tons, ſhould contain 15 tons of water, and only 1 ton of ſalt; yet it may chance, that ſuch a weak brine may be wrought with more profit than the ſtrongeſt; for the profit ariſing from the boiling of brine into ſalt, depends as much upon the price of the fuel uſed in boiling it, as upon the quantity of ſalt which it yields. Thus the ſea water, which ſurrounds the coaſts of Great Britain, is ſaid to hold ſeldom more than one thirtieth, or leſs than one fiftieth part of common ſalt; but fuel is ſo cheap at Newcaſtle, that they can evaporate thirty or forty tons of water, in order to obtain one ton of ſalt, and yet gain as much clear profit as thoſe do, who, in counties leſs favourably ſituated for fuel, boil down the ſtrongeſt brine.

The advantage reſulting from ſtrengthening weak brine or ſea water, by means of rock ſalt, is very obvious. Suppoſe that the ſea water at Liverpool, where large quantities of rock ſalt are refined, would yield 1 ton of ſalt from 48 tons of water, then muſt a quantity of fuel ſufficient to evaporate 47 tons of water be uſed in order to obtain 1 ton of ſalt. But if as much rock ſalt be put into the 48 tons of ſea water, as can be diſſolved in it, then will the ſea water reſemble a brine fully ſaturated, each 16 tons of which will give 4 tons of ſalt, and the whole quantity yielded by the evaporation of 47 tons of water, will be 12 tons of ſalt.

In warm climates, ſuch as *Italy*, *Spain*, and the ſouthern provinces of *France*, where the heat of their ſummer is ſufficient to exhale conſiderable depths of water in a little time; they prepare large quantities of what is called *bay ſalt*, at a very ſmall expence. Their general method is to ſelect a marſhy ſpot of ground, communicating by ſluices with the ſea, and ſufficiently removed from freſh ſprings, and the mouths of large rivers. In this ground they form uſually three pits of ſmall but different depths, and by clay and timber they ſecure them from leaking. The ſea water is let into the firſt and deepeſt pit, from which it flows ſlowly, ſo as to give the mud and ſand time to ſettle, through the ſecond into the third, where it is ſoon inſpiſſated by the heat of the ſun into a ſtrong brine. This brine being ſtill further evaporated by the continued action of the ſun, concretes into cryſtals of ſalt, which gradually ſink to the bottom of the pit. When a proper quantity of it is formed, it is drawn out for uſe, and is of different colours, according to the colour of the clay employed in making the pits. In a favourable ſeaſon, the French are ſaid to be able to make enough of bay ſalt in a fortnight, to ſupply their home conſumption for a year, and the wants of the other nations, who purchaſe from them much more than is conſumed in France*.

In this method of making bay ſalt, art has but copied nature; for we read in Pliny, Alonſo Barba, Shaw, and others, of large briny lakes, which are evaporated, in many parts of the world, to a perfect dryneſs in ſummer time; when the inhabitants cut and carry away as much ſalt as they have occaſion for. A remarkable lake of this kind was met with by Mr. Maſſon, in his botanical excurſions from the Cape of *Good Hope*. "*Zwart Kop*'s ſalt-pan, is a lake ſeveral

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[*] See Dr. Brownrigg's Treatiſe on the Art of making common Salt, where the reader's curioſity on this ſubject will be abundantly gratified.

veral miles diſtant from the ſea, and upon an eminence. In the rainy ſeaſon it is filled with freſh water, which by the ſaltneſs of the ground, ſoon becomes ſtrongly impregnated with ſaline particles; and when the ſummer's heat exhales the freſh water, the bottom of the lake is covered with a cruſt of pure ſalt, two or three feet thick. The lake is about three miles round, and ſurrounded by a riſing ground*."

Though it appears, from what Dr. *Brownrigg* has ſaid upon this ſubject, that we might, with much advantage to our trade, imitate the French in their manner of procuring bay-ſalt; yet moſt of our ſalt is prepared by boiling either the brine of ſalt ſprings, or ſea water, into white ſalt,

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* Philoſ. Tranſ. 1776. p. 297.

ſalt. In Hampſhire, and ſome other counties, they have a method of heightening the ſea water into a ſtrong brine, by expoſing it to the ſun in large pits, much after the manner in which bay ſalt is made; and ſo much of the ſea water is, by this means, naturally evaporated, that though they boil the remainder with Newcaſtle coal, yet they can afford their ſalt at a leſs price, than the Newcaſtle ſalt boilers can afford theirs; though the ſea water is, probably, equally impregnated with ſalt at both places.

The greateſt difficulty which attends the making of bay ſalt in Great Britain, ariſes from the heat of our ſummer not being ſufficiently ſtrong, to evaporate a great quantity of ſea water in a ſmall portion of time. I will mention an experiment or two, which ſuggeſted a hint, that may not, perhaps, be unſerviceable to thoſe who ſhall, in future, be diſpoſed to attempt the making of bay ſalt.

I took a piece of coarſe linen cloth; its area was a ſquare yard, and having thoroughly wetted it with water, but not ſo much as to cauſe it to drip, I weighed it, and in that wet ſtate expoſed it to a warm ſun in June, when there happened to be but little wind; it was not laid upon the ground, but held oppoſite to the ſun, ſo that the air and ſun had a free acceſs to every part of it; it became nearly dry in an hour, and in that ſpace of time it had loſt eight ounces of its weight. — Upon another occaſion, when there was a bright ſun and a briſk wind, on an April day, a ſimilar experiment was made; and the cloth loſt after the rate of 6 ounces in one quarter of an hour; hence, if it had been kept conſtantly wetted, it would have loſt in 16 hours 24 pounds; a pound of water is equal to a pint in meaſure, and therefore we may infer, that in a ſummer day of 16 hours in length, 24 pints, or three gallons of water may be diſperſed into the air, by a warm ſun and a briſk wind, from the ſurface of a linen cloth, equal to one ſquare yard.

Now it is very poſſible, in any place where bay ſalt is made, to deviſe means of wetting and drying alternately any number of ſquare yards of coarſe cloth; one labourer, aſſiſted by proper mechanical contrivances, might be equal to the daily management of a thouſand yards, or more. By this plan, in favourable weather, there would be daily evaporated twenty-four thouſand pounds of water, which, ſuppoſing ſea water to contain one thirty-ſecond part of its weight of ſalt, would give 750 pounds of ſalt. Thoſe who have ſeen the artifice of ſtrengthening brine, which is practiſed in *Franche-Comtè*, and other places, by making it drip through faggots, in order that the evaporation may be increaſed, by increaſing the ſurface of the water which is expoſed to the air, will not be ſurpriſed at the method of evaporating water here hinted at.

Though the conſtituent parts of common ſalt, when free from all heterogeneous mixtures, are probably the ſame in every part of the world, yet in the ordinary ſtate in which it is met with, it is found to have very different qualities, and may properly enough be divided into different ſorts. Bay ſalt differs in ſome of its properties from the white ſalt, prepared by boiling brine or ſea water; and both the bay ſalt and the boiled ſalt have, reſpectively, different qualities, according as they are prepared at different places. There are two general reaſons of this diverſity; one reſpects the manner of preparing the ſalt, the other reſpects the quality of the water from which the ſalt is prepared. When ſea water or brine is boiled into ſalt, a portion of the acid, which is one of the conſtituent parts of the ſalt, is diſperſed; and a greater or leſs portion is diſperſed, according as the ſalt has been formed with a greater or leſs degree of heat. We have an inſtance of this, both in bay ſalt when compared with boiled ſalt, and in the different ſorts of boiled ſalt, when compared with each other. Bay ſalt is prepared from ſea water, by the mild heat of the ſun, and it is generally eſteemed much ſtronger than the white ſalt, which is prepared likewiſe from ſea water by boiling the ſea water. At Northwich, and in other places, they prepare from brine, not only the ordinary white ſalt, but a ſalt whoſe cryſtals are much larger than the ordinary ſalt, and which is uſually called ſhivery ſalt. This ſhivery ſalt is eſteemed ſtronger than the other, and it is formed by a milder heat; for in forming the ordinary ſalt, the brine is kept gently boiling; but in forming the large grained ſhivery ſalt, they let the brine cool till they can bear to put their hands into it, and keeping it in that ſtate, as near as may be, from Saturday night to Monday morning, the ſhivery ſalt is gradually formed. Sea water, brine ſprings, and rock ſalt, generally contain beſides common ſalt, various other earthy and ſaline ingredients, ſuch as the *calcareous earth* from which fiſh ſhells are probably formed;—the earth called *magneſia*;—*Epſom ſalts*, or the ſalt reſulting from the combination of the acid of vitriol with magneſia;—*Selenites*, or the ſalt reſulting from the combination of the acid of vitriol with the earth of the nature of fiſh ſhells;—*Glauber's ſalt*;—*fixed alkali* uncombined with any acid, &c. Sometimes all theſe heterogeneous ſubſtances, and ſometimes only a few of them are found in the waters from which common ſalt is prepared; they are all of them foreign to the nature of the ſalt, and injure its quality; and hence we may, without difficulty, comprehend that common ſalt may have very different properties, according to the quality of the water from which it is made, or the ſkill of the ſalt-maker exerted in ſeparating theſe mixtures from it. The *Dutch* have long been famous for preparing a ſalt for the pickling of herrings, by which they have acquired a ſuperiority in that article of commerce over all other European nations. Their principal ſecret in this buſineſs conſiſts in evaporating the brine made from the ſolution of bay ſalt with the gentleſt fire, and in mixing with the brine a proper quantity of very *ſour whey*; the acid whey unites itſelf with the uncombined fixed alkali before mentioned, and thus prevents it from adhering to the common ſalt as it cryſtallizes. Any other mild acid might probably anſwer the ſame purpoſe. This acid whey is called *azy*: les Hollandois ſe ſervent de l'*azy* dans le travail en grand pour raffiner nos ſels de mer, et pour en rendre la criſtalliſation parfaite; c' eſt par cet artifice qu'ils ſont depuis long-temps, en poſſeſſion de fournir les meilleures ſalaiſons de l' Europe*.

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* Hiſt. de l'Acad. des Scien. 1762. Dr. Brownrigg had mentioned this ſecret of the Dutch above 30 years ago, in his excellent treatiſe before referred to. The reader will

meet with an accurate examination of the various ingredients with which common ſalt, and the waters from which it is made, are impregnated in Baumé's Chem. vol. iii. p. 512.

ESSAY III.

OF COMMON SALT AND NITRE AS MANURES.

COMMON ſalt, in the earlieſt ages of the world, was ſo far from being held in any eſtimation as a manure, that it was looked upon as a ſymbol of extreme ſterility. Long before the commencement of profane hiſtory, *Abimelech* took the city *Shechem*, and *ſlew the people that was therein, and beat down the city, and ſowed it with ſalt*[*]; intimating thereby by his wiſh that it might remain unfruitful and uninhabited for ever.

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[*] Judges ix, 45.

It does not appear from hiſtory, what circumſtance gave riſe to this cuſtom, or whether Abimelech was the firſt perſon who introduced it; but it has prevailed very generally among mankind even to the preſent age, it being ſtill uſual to raze the houſes of rebels and traitors, and to ſprinkle the ground upon which they ſtood with ſalt. Perhaps this general ſymbolical uſe of ſalt, might be derived from what had happened when *Sodom* was deſtroyed, and the *ſalt ſea* was formed. This conjecture is, in ſome meaſure, ſupported by obſerving, that the ſevereſt denunciations of divine vengeance are frequently expreſſed by an alluſion to that event.—*Moab ſhall be as*

Sodom, and the children of Ammon as Gomorrah, even the breeding of nettles, and ſalt pits, and a perpetual deſolation[*]. *The whole land thereof is brimſtone, and ſalt, and burning, that it is not ſown, nor any graſs groweth therein, like the overthrow of Sodom*[†].

Virgil reprobates a ſalt ſoil, as occaſioning the degeneration of fruit trees, and admitting no melioration from ploughing.

> *Salt* earth and bitter are not fit to ſow,
> Nor will be tam'd or mended with the plough.
> Sweet grapes degen'rate there, and fruits declin'd
> From their firſt flav'rous taſte renounce their kind[‡].

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[*] Zephaniah, ii. 9.

[†] Deuteron. xxix. 23.

[‡] Virgil's Georg. l. 2. Dryd.

Pliny, though he recommends the giving of ſalt to cattle, yet in ſpeaking of foſſile ſalt, he expreſsly affirms, that every place in which it is found, is barren and unfit for vegetation*.

Notwithſtanding theſe and other teſtimonies of the ancients concerning the ſterilizing quality of common ſalt, yet it is frequently uſed as a manure with great ſucceſs. It has been the cuſtom, ever ſince the time of Henry the Third at leaſt, for the farmers on the *Corniſh* coaſt, to manure their lands with ſea ſand, in which ſea ſalt is ſo copiouſly mixed, that in many places it uſed to be extracted from a lye made of ſand. When the ſand has been long expoſed to the air, it proves leſs uſeful and enriching, which is by ſome attributed to its having been deprived of a good part of its ſalt, by the dews and rains*. It may be obſerved, that this ſand is of two kinds, ſhelfy and ſhelly; the ſhelfy is produced from the grating of ſtones one againſt another, by the conſtant waſh of the ſea, and is of little value; its quality, however, will be different in different countries, according to the nature of the ſtones expoſed to the agitation of the ſea; if the ſtones happen to be of the limeſtone kind, even the ſhelfy ſand may be very ſerviceable; if the ſtones principally conſiſt of flint, as at Yarmouth, and other parts of the Norfolk coaſt, the ſand will differ very little from powdered flints. The ſhelly ſand in Cornwall,

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* Hiſt. Nat. L. xxxi. c. 7.

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* Camden's Britan. Cornw.

is produced by the ſame fluctuation of the ſea, as the flinty ſand in Norfolk; it conſiſts of the broken ſhells of muſcles, cockles, oyſters, ſcollops, and other fiſh. This ſand varies ſomewhat in colour and in grain, according to the ſubſtances from which it is formed, and the degree of agitation it has been expoſed to, and it is found to fertilize almoſt all kinds of ſoils*. This practice of manuring lands with ſea ſand, has, within theſe few years, been introduced with great ſucceſs in other parts of Great Britain.

The *Cheſhire* farmers purchaſe no inconſiderable quantity of refuſe ſalt from the ſalt boilers; they mix it with dung, and it makes an excellent manure. This refuſe ſalt coſts them eightpence a buſhel, there being a duty of fourpence a buſhel impoſed on it. When the demand for this refuſe ſalt is very great, the ſalt boilers have a cuſtom to mix a quantity of ſoot, or other impurity, with the ordinary ſalt, and it then comes under the denomination of refuſe ſalt; there were ſold a few years ago, at Northwich alone, near 3000 tons of it in one year, for the uſe of the farmers in that and other counties. On the other hand, when the ſoil abounds with ruſhes and weeds, it is cuſtomary in Cheſhire, to lay a quantity of rock ſalt upon it, as it is found utterly to deſtroy every vegetable. Hence it ſhould ſeem, that ſalt, when uſed in ſmall quantities, is a good manure, and when in large ones, a real poiſon to vegetables. Some of the African and

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* Philoſ. Tranſ. No. 113.

Arabian deſerts are thought to be barren by their having too much ſalt in them; whilſt many parts of Barbary are reckoned to be peculiarly fruitful, from their containing a leſs quantity of it*.

Sir *John Pringle* has obſerved, that common ſalt, when uſed in ſmall quantities, as one ounce to twelve, accelerates the putrefaction of animal ſubſtances, when in larger it retards it; and hence he deduces its utility, in aſſiſting the organs of digeſtion in man, and, perhaps, in other carnivorous animals†. The ſame obſervation may be extended to graminivorous animals, they are all exceedingly fond of ſalt, probably from its great tendency to corrupt the aliment, and to render thereby its diſcharge from the ſtomach into the inteſtines, more eaſy and expeditious.

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* Brownrigg on Salt, p. 159.

† Obſerv. on the Diſeaſes of the Army, by Sir J. Pringle, Appen. xxxix.

In like manner ſalt, when applied as a manure, in ſmall quantities, is found to be very beneficial; not probably from its entering as an aliment into the ſubſtance of vegetables, ſince there are many experiments tending to prove that no kind of ſalt can of itſelf become the food of plants*, but from its efficacy in reducing weeds, dried herbage, dead roots, &c. into a putrid oily maſs; the fructifying virtue of oily compoſts being now generally acknowledged: but when it is uſed in a larger proportion, by preſerving theſe matters from corruption, and drying up or hardening the fibrous capillaries of the roots, ſo that they become unfit for ſucking in nutriment, the fertility

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* Wallerius expreſsly ſays—Les ſels de quelle eſpece qu' ils ſoient, *ne peuvent pas ſervir de nourriture aux plants, ni avancer par eux-memes la vegetation*; and after quoting the experiments of Kraftius, Alſton, and Bonnet, on the ſubject, and examining the arguments on the oppoſite ſide of the queſtion, he concludes the inquiry in the following words:

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words: — D'ou nous concluons que les ſels minéraux, ni les ſels étrangeres, ni les terres minérales, ne contribuent point a la nourriture des végétaux. Elemens d'Agriculture traduits du Latin de M. Wallerius. Yverden, 1766, p. 134, &c. or an Engliſh tranſlation of the ſame work, by John Mills, Eſq. F. R. S. London, 1770. See alſo the Principles of Agriculture and Vegetation, by Dr. Home; and the Elements of Agriculture, by Dr. G. Fordyce.

lity of the ground is diminiſhed, or wholly deſtroyed.

As to the fertility of lands overflowed by ſea water, it may be in part owing to the ſlime and mud left by it, and partly to the ſalt contained in it, which, being in a ſmall quantity, may contribute to the putrefaction of the effete vegetable roots, and the conſequent production of an oily compoſt.

With reſpect to nitre as a manure, it is an opinion entertained by farmers, that ſnow fertilizes the lands upon which it falls, more than rain, in conſequence of the nitrous ſalts which it is ſuppoſed to acquire by freezing. Falſe philoſophy firſt gave riſe to this idea*, and poetry has contributed to ſpread the general error.

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* Gaſſend. Phyſ. ſec. iii. l. 11. The exiſtence

——Oh! may'ſt thou often ſee
Thy furrows whiten'd by the woolly rain
Nutritious, ſecret *nitre* lurks within
The porous wet, quick'ning the languid glebe.

PHILIPS.

What art thou froſt?——
Is not thy potent energy, unſeen
Myriads of little *ſalts*, or hook'd or ſhap'd
Like double wedges, and diffus'd immenſe
Thro' water, earth, and ether.

THOMPSON.

The

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iſtence of nitre in ſnow, and the fertilizing quality of ſalts, were ſo generally admitted in the laſt century, that philoſophers ſeem to have been at no pains to prove either of them, — On *ſait* que la *neige* contient beaucoup de parties *nitreuſes*, et que les *ſels* contribuent beaucoup à la *fecondité* des terres. Actes de Copenhague, Collect. Acad, vol. vi. p. 195.

The ancients were acquainted with the fertilizing quality of ſnow, and though they did not very intelligibly explain its mode of operation in promoting vegetation, yet it is remarkable, that the ſuppoſition of ſaline corpuſcules made no part of their illuſtration of that matter*. It is not here intended to deny the fertilizing virtue of ſnow, but to ſhew that it probably does not proceed from the nitre contained in it; in ſupport of this opinion, I will mention two of the moſt accurate experiments, which have probably ever been made on the ſubject.

Marggraf, in the year 1751, collected in the ſuburbs of Berlin as much ſnow as, when melted, afforded him one hundred meaſures of water, each meaſure containing thirty-ſix ounces*. He collected the ſnow in an open ſituation, in clean glaſs veſſels, after the atmoſphere was purified, by its having ſnowed ſome time; in ſhort he uſed every poſſible precaution to procure the ſnow free from every extraneous impurity. The nitrous or other ſalts contained in the ſnow as it fell upon the ground were, no doubt, diſſolved in the water after the ſnow was melted; and, in order to aſcertain their quantity and quality, nothing more was requiſite than to diſſipate by evaporation or diſtillation, the water in which they were diſſolved. From theſe hundred meaſures of ſnow water he obtained, by diſtilling it with every ſuitable attention, 60 grains, not of *nitre*, but of *calcareous earth*, together with ſome grains, he does not mention the exact number, of the acid of ſea ſalt, impregnated with a nitrous vapour.

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* Plin. Hiſt. Nat. l. xvii. c. 2.

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* Opuſ. Chy. vol. ii.

The ſame quantity of rain water, collected in the winter months with equal precaution, and diſtilled with equal attention, yielded 100 grains, not of *nitre*, but of *calcareous earth*, with ſome grains of the acids of nitre and ſea ſalt*.

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* Toute la difference donc entre l' eau de luie et l' eau de neige n' eſt d' aucune imporance, et ſe reduit à ce que l' acide de l' eau e pluie eſt plus *nitreux*, et qu' elle renferme lus de terre calcaire, au lieu que l' eau de eige a plutôt un acide *marin que nitreux*, et ontient une moindre quantité de terre calcaire. Opuſcules Chymique, vol. ii. p. 21.

The author infers from theſe experiments, that the chemical difference between rain and ſnow water is exceedingly ſmall; but that the latter, however, is ſomewhat leſs nitrous, and contains a ſomewhat leſs proportion of earth than the former. But neither of them contains either earth or any kind of ſalt in any quantity, which can be ſenſibly efficacious in promoting vegetation.

Suppoſe an acre of land to be, at any one time, covered with ſnow to the depth of ſix inches; and that this ſnow would, when melted, be reduced in magnitude a ſixth part, or that it would cover the land with water to the depth of one inch; then might it be made appear, by a very eaſy calculation, that allowing 6 grains of calcareous earth to every

28 gallons and one pint of water, according to the proportion of the foregoing analyſis, the whole of the earth left upon the acre from the evaporation of the ſnow water, would not amount to ten pounds in weight; nor the ſaline part to above a few ounces. Now it is very improbable that four or five ounces of any kind of ſalt, when ſpread over an acre, ſhould produce any ſenſible effect in fertilizing the ground. And if we uppoſe the land to be ſo often covered with ſnow in the courſe of a ear, that the ſalt left upon it would mount even to a pound, yet the ifficulty would ſtill recur, ſince ven a pound of ſalt reduced to an niform pellicle, and ſpread over an cre of land, would not much exceed in thickneſs, the four hundred

thouſandth part of an inch. So that if it ſhould be admitted that nitre is a fertilizer of land, which many are, upon good grounds, diſpoſed utterly to deny, yet ſo very ſmall is the quantity of it contained in ſnow, that it cannot be ſuppoſed to promote the vegetation of plants upon which the ſnow has fallen.

The peculiar agency of ſnow as a fertilizer, in preference to rain, may admit of a very rational explanation, without having recourſe to any nitrous ſalts, which it has been ſuppoſed to contain. There are two cauſes conſtantly acting upon the ſurface of the earth, one of which is, generally ſpeaking, uniform in its ſtrength with reſpect to the production of heat; the other is variable. The internal ſubſtance of the earth is heated uniformly, by ſome principle or other which we do not well underſtand, to the 48th degree of Fahrenheit's thermometer; this degree of heat is greater than that in which the watery juices of vegetables freeze, and it is propagated from the inward parts of the earth to the ſurface on which the vegetables grow. The atmoſphere being variably heated, by the variable action of the ſun in different climates, and in the ſame climate at different ſeaſons, communicates to the ſurface of the earth, and to ſome diſtance below it, the degree of heat or cold which prevails in itſelf. Different vegetables are able to preſerve life under different degrees of cold; but all of them periſh, when the cold which reaches their roots is extreme. Providence, in the coldeſt climates, has provided a covering for the roots of vegetables, by which they are protected from the influence of the atmoſpherical cold; that *covering* is *ſnow.* Even in our climate we often ſee ſhrubs, and other ſmall vegetables, killed by the ſeverity of a froſt, when it is unaccompanied with ſnow, which, had there been eight inches, or a foot of ſnow upon the ground, would have remained unhurt. The ſnow keeps in the internal heat of the earth, and it does not permit the cold of the atmoſphere to penetrate through its ſubſtance, and on both theſe accounts it keeps the roots of the vegetables, comparatively ſpeaking, warm; a kind of vegetation is carried on under its ſurface, which ſhews itſelf by the very rapid increaſe which plants of all kinds acquire in cold countries, as ſoon as the covering of ſnow is removed by the approach of ſpring. Thoſe who are fond of making oil the food of plants, may derive part of the utility of ſnow in fertilizing the earth from the oily particles which it contains; for not only the reſidue which was obtained from the diſtillation of ſnow water, had a brown colour proceeding from ſome oily particles, but it is very commonly obſerved that ſnow, as it thaws, becomes dirty; and this dirty appearance is probably enough referred to ſome particles of an oily or a mucilaginous ſubſtance contained in it. Snow, moreover, in melting, moiſtens and pulverizes the ſoil, which had been bound up by the froſt; and as its water has a tendency to putrefaction, it ſeems, on many accounts, without admitting it to contain any nitre, to be admirably fitted to promote vegetation.

In confirmation of the analyſis of rain and ſnow water which were made by Marggraf, I will here add thoſe which we owe to the accurate induſtry of Dr. Rutty*.—He evaporated ſix different ſpecimens of the pureſt rain water he could procure, and found that a gallon of the pureſt yielded ſix grains, and a gallon of the moſt impure 12 grains of a ſubſtance, for the moſt part, of a dark brown colour, and a *bitter, brackiſh* taſte. From a gallon of ſnow water, he obtained 4 grains of a browniſh reſidue, which conſiſted partly of *ſea ſalt*, partly of an abſorbent earth, and a ſmall portion of oil.

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* Rutty's Synop.

It may be obſerved from theſe experiments, compared with thoſe of Marggraf, that the proportion between the ſolid impurities of rain and ſnow water, is much the ſame, in parts of the globe as far diſtant from each other as *Ireland* and *Pruſſia*. Dr. Rutty's experiments were made in Dublin; a gallon of the pureſt rain water gave him 6 grains, the ſame quantity of the pureſt ſnow water gave him 4 grains of heterogeneous matter; now the proportion of 6 to 4, is not very different from that of 100 to 60, which expreſſes the proportional quantities of the reſidues left, from the evaporation of rain and ſnow water, according to the experiments of Marggraf.

No two authors perfectly agree in aſcertaining the quantity of ſolid impurity contained in a definite portion of either rain or ſnow water; this is not to be wholly attributed to the difference of their care and ſkill exerted in the analyſis of them, but principally to the different natures of the ſubjects themſelves, upon which their labours have been employed. The vapours which are daily raiſed from the ſea, may, with great probability be ſuppoſed to carry with them ſome minute portions of the acid of ſea ſalt, and, perhaps, of ſea ſalt in ſubſtance, and of the calcareous earth, with which the ſea abounds. The atmoſphere itſelf may be looked upon, not only as the general receptacle ceptable of theſe aqueous vapours, but likewiſe of all mineral exhalations; of the ſteams, which are conſtantly ariſing from the perſpiration of whatever enjoys animal or vegetable life; and from the inſtantaneous putreſcence of thoſe ſubſtances, when deprived of life; of the ſmaller ſeeds of terreſtrial and aquatic plants; of the eggs of an infinity of ſpecies of imperceptible animalcules; of the acids and oils ſeparated by combuſtion from all ſorts of fuel; of the matter of light; of electric effluviums; and of a variety of other ſubſtances, which are elevated, and, for a time kept ſuſpended, by natural and accidental cauſes: from theſe ſources are derived, thoſe various impurities, which have been diſcovered in all atmoſpherical water; and which we can readily apprehend, muſt be different in quality and quantity, according to the nature of the heterogeneous ſubſtances ſubſiſting in that portion of the atmoſphere, through which the aqueous vapours have aſcended, or which they have waſhed in their deſcent; that is, in general, according to the nature of the climate, the ſeaſon of the year, the direction of the winds, and the preceding temperature of the weather.

ESSAY IV.

OF THE SALTNESS AND TEMPERATURE OF THE SEA.

THERE are few queſtions, reſpecting the natural hiſtory of our globe, which have been diſcuſſed with more attention, or decided with leſs ſatisfaction, than that concerning the primary cauſe of the ſaltneſs of the ſea. The ſolution of it had perplexed the philoſophers before the time of Ariſtotle*; it ſurpaſſed

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* Ariſt. Meteo. L. ii. c. iii

paſſed his own great genius; and thoſe of his followers, who have attempted to ſupport his arguments, have been betrayed into very ill-grounded concluſions concerning it. Father Kircher*, after having conſulted three and thirty authors upon the ſubject, could not help remarking, that the fluctuations of the ocean itſelf were ſcarcely more various, than the opinions of men concerning the origin of its ſaline impregnation.

The queſtion does not ſeem capable of admitting an illuſtration from experiment; at leaſt, no experiments have hitherto been made for that purpoſe, and therefore we may be the leſs ſurpriſed at its remaining nearly as problematical in the preſent age, as it has been in any of the preceding.

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* Kirch. Mun. Subter. L. iii. c. iii.

ceding. Had there, indeed, any obſervations been made, three or four centuries ago, aſcertaining the then ſaltneſs of the ſea, at any particular time and place; we might, by making, at preſent, ſimilar obſervations at the ſame place, in the ſame ſeaſon, have been able to know whether the *ſaltneſs* at that particular place, was an *increaſing*, or a *decreaſing*, or an *invariable* quantity: and this kind and degree of knowledge would have ſerved as a clue to direct us to a full inveſtigation of this matter in general; but it is to be regretted, that no ſuch obſervations have, till very lately, been made with any tolerable preciſion.—There are three principal opinions on this ſubject, which have been maintained by philoſophers of modern date.

Some * obſerving that river water, almoſt in every part of the globe, is, in a greater or leſs degree, impregnated with ſea ſalt, have thought that the ſea has gradually acquired its preſent quantity of ſalt from the long continued influx of rivers. The water, which is carried into the ſea by the rivers, is again ſeparated from it by evaporation, and being diſperſed over the atmoſphere by the winds, it ſoon deſcends in rain or vapour upon the ſurface of the earth; from whence it haſtens to pour into the boſom of the ocean, the freſh tribute of ſalt, which it has collected in its inland progreſs. Thus the ſalt conveyed into the ſea, not being a volatile ſubſtance, nor performing an inceſſant circulation, like the water

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* Halley, Philoſ. Tranſ. No. 344.

ter which carries it thither, muſt be a perpetually increaſing quantity, and time enough, it is contended, has elapſed ſince the creation, for the ſea to acquire from this ſource its preſent quantity of ſalt.

The principle upon which this opinion is founded, cannot become the ſubject of accurate calculation; rivers may have formerly carried more or leſs ſalt into the ſea, than they do at preſent; and the quantities, contained at preſent in different rivers, are ſo various, that any general poſition, with reſpect to the proportion of ſalt obſervable in river water, will be liable to many exceptions from particular caſes. It may appear probable, however, from the following obſervation, that the cauſe here aſſigned of the ſaltneſs of the ſea, is not adequate to the effect. Sea water, at a medium taken from obſervations made in different climates, may be ſuppoſed, without any great apprehenſion of error, to contain about one thirtieth of its weight of common ſalt; but all the rivers in the world could not have conveyed into the ſea one five hundredth part of the weight of the ſea, though we ſuppoſe that they have daily, ever ſince the creation, carried into the ſea and left there, the ſame quantity of ſalt, which they do at preſent daily depoſit in it. In order to make this out, with any appearance of perſpicuity and preciſion, we will take for granted, what ſome eminent philoſophers have endeavoured to prove, — " that all the rivers in the world flowing into the bed of the ſea, with a continuance of their preſent ſtores, would take up at leaſt eight hundred years to fill it to its preſent height*;"—this being premiſed, let us ſuppoſe the ocean to have been originally quite freſh, and to have daily loſt by evaporation as much water, as it daily received from the influx of the rivers, then muſt eight hundred years have paſſed before the freſh water of the ocean could have been changed into river water, or acquired that proportion of ſalt with which river water is impregnated. River water may, in general, be ſuppoſed to contain one four thouſandth part of its weight of ſea ſalt; ſome rivers, doubtleſs, contain

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[*] Goldſmith's Hiſt. of the Earth, &c. Vol. I. p. 228. Buffon, Hiſt. Nat. Vol. I. p. 356.

tain more, others leſs, but the quantity here aſſumed ſeems to be great enough; ſince even the Thames* water, taken up near Billingſgate, impregnated, as it may be ſuppoſed to be, with ſea ſalt, from the quantity of it uſed in London, and waſhed into it by various drains, does not contain one half ſo much; the ocean upon this hypotheſis, at the expiration of eight hundred years from the creation, would have contained one four thouſandth part of its weight of ſea ſalt; and at the expiration of eight times eight hundred years, it would have contained eight four thouſandth parts, or one five hundredth part of its weight of ſalt; but eight times eight hundred years carry us back to an æra antecedent to the creation of the world.

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* See an ingenious Inquiry into the Nature of Water, by Dr. Rotherham, p. 114.

Other philoſophers *, obſerving that large beds of foſſile ſalt are not unfrequent in any quarter of the globe; and conceiving, with great probability, the bottom of the ſea to be analogous in its formation to the ſurface of the earth, have undertaken to derive its ſaltneſs from the beds of rock ſalt, which they have ſuppoſed to be ſituated at its bottom; and they are further of opinion, that without ſuch a permanent ſaline principle, the ſea would long ſince have become inſipid from the freſh water poured into it by an infinity of rivers. Strange! that what,

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* Mem. de l' Acad. des Scien. de Ber. Ann. 1760.

what, according to the forementioned hypotheſis was thought ſufficient to account for the ſaltneſs of the ſea, ſhould in this be eſteemed inſtrumental in annihilating the ſaltneſs already ſuppoſed to exiſt.

This opinion is liable to an objection of ſome weight; for it may reaſonably be inquired, why the waters of the ocean are not perfectly ſaturated with ſalt, if they have ever ſince the creation been exerting their diſſolving powers upon ſuch permanent maſſes of rock ſalt as are thought to be ſituated at its bottom. Were the waters of the ocean in a quieſcent ſtate, and every where of two or three miles in depth, as they are conjectured to be in ſome, it would not be an eaſy matter to define the time in which the waters at the ſurface would become ſapid, much leſs, in which they would become ſaturated, though the whole bottom of the ocean was covered with a ſtratum of rock ſalt. The water contiguous to the ſalt would ſaturate itſelf therewith, and being thereby rendered heavier than the ſuperficial water, it would not readily mix itſelf with it; but though the lower parts of the ſea have been obſerved by divers to remain in a very tranquil ſtate, when the upper have been much agitated by ſtorms; yet it may reaſonably be ſuppoſed, that they are ſufficiently moved, to cauſe, in a ſufficient length of time, an uniform diffuſion of the rock ſalt, they may be ſuppoſed to have diſſolved. The patrons of this opinion found their reaſoning upon a miſtake;

they think, that if there were no mines of rock ſalt in the bed of the ocean, its waters would have been long ago converted into freſh water, by the rivers diſcharged into it. Now this is an erroneous principle; for ſuppoſing that the rivers themſelves contained no ſalt, and that they returned into the ſea, in any given time, the ſame quantity of water, which in the ſame time is raiſed from it by evaporation, the ſaltneſs would remain preciſely the ſame for ever. But neither of theſe ſuppoſitions is true, for the rivers do convey into the ocean ſome portion of ſalt every day, and they do not, in any given time, return into the ocean ſo much water as in the ſame time is ſeparated from it; and upon both theſe accounts, the ſaltneſs of the ſea, ſo far from being diminiſhed, ought conſtantly to be increaſed, from the rivers poured into it.

Boyle * unites, as it were, the two preceding hypotheſes, and "takes the ſaltneſs of the ſea to be ſupplied, not only from rocks and other maſſes of ſalt, which at the beginning were, or in ſome countries may yet be found either at the bottom of the ſea, or at the ſides, where the water can reach them; but alſo from the ſalt which the rivers, rains, and other waters diſſolve in their paſſage through divers parts of the earth, and at length carry with them into the ſea." Buffon †, and the generality

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* Boyle's Works, Vol. III. p. 381.

† Hiſt. Nat. Vol. I. p. 361. Goldſmith's Hiſt. of the Earth, Vol. I. p. 234.

rality of philoſophers, acquieſce in the opinion of Boyle.

After all, it may be obſerved, that we are inquiring into the cauſe of a phænomenon, which it may be ſaid had no ſecondary cauſe at all. It is taken for granted in this diſquiſition, that the water which covered the globe in its chaotic ſtate, was not impregnated with ſalt as at preſent; but quite freſh; now this is an opinion concerning a matter of fact, which can never be proved either way; and ſurely we extend our ſpeculations very far, when we attempt to explain a phænomenon, primeval to, or coeval with, the formation of the earth.

Bernardine Gomeſius *, about two hundred years ago, publiſhed an ingenious treatiſe upon ſalt: in this treatiſe, after reciting, and refuting, the opinions of Empedocles, Anaxagoras, and Ariſtotle, upon the ſubject in queſtion, he propoſes his own; wherein he maintains that the ſea was originally created in the ſame ſtate in which we at preſent find it, and impregnated from the very firſt with the ſalt which it contains. Though this hypotheſis may be conſidered by ſome, rather as a cutting than any untying of the knot, yet it has been embraced by philoſophers of great eminence*; and it muſt be owned that it may be applied to the ſolution of ſome phænomena with peculiar propriety. Naturaliſts aſſure us, that, though ſome few ſpecies of fiſhes thrive in freſh water, and ſome others live alternately in freſh water and ſalt, yet by far the greateſt number cannot exiſt out of the ſea: now whether we ſuppoſe the ſea to have become ſalt from the influx of rivers, or from the gradual ſolution of beds of rock ſalt, or from the combined influence of both theſe cauſes, it muſt for ſome years have remained ſo exceedingly freſh, that it will not be an eaſy matter to account for the continuation of the exiſtence of the numberleſs ſpecies of fiſhes, which cannot live in freſh water. This difficulty is not removed by ſuppoſing that fiſhes do not imbibe any part of the ſea's ſaltneſs with their food, and attributing the efficacy of ſea water in preſerving life, to the ſuperior weight with which it compreſſes the organs of reſpiration, for this ſuperior weight is as much an effect of the ſalt diſſolved in it, as the ſaline taſte itſelf. The ſaltneſs of the *Caſpian* ſea, of the lakes of *Mexico*, and *Titicaca*, and of other large collections of waters, which have no effluent rivers, nor viſible communication with the ſea, may be as ſucceſsfully explained upon this hypotheſis,—that the ſea was at the creation impregnated with ſalt — as upon either of the preceding.

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* Gomes de Sale, l. i. c. 24.

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* Kircher Mun. Sub. L. iii. c. iii.——Nollet. Lec. de Phyſ. Tom. IV. p. 62.

Beſides the opinions, concerning the cauſe of the ſaline impregnation of the ſea, which have been here mentioned, there is another, which future ages will, probably, ſee leſs queſtionable reaſons to adopt than we do: I mean that which maintains —that ſea ſalt is conſtantly and abundantly generated, both on the ſurface of the earth, and in the boſom of the ocean.

But how ineffectual ſoever our attempts may be to explain the cauſe of the ſaltneſs of the ſea; yet one might have hoped, that in this age of philoſophy and curious navigation, the degree of its ſaltneſs in every latitude, and every ſeaſon of the year, would have been aſcertained by accurate experiments. The acquiring knowledge by experiments, is a ſlow and laborious method, but it is, at the ſame time, a method within our reach: whilſt the theoretical inveſtigation of the proximate cauſe of any natural phænomenon often ſurpaſſes, and that of its ultimate cauſe always ſurpaſſes the apprehenſion of the human intellect.

Some experiments tending to the elucidation of this point, reſpecting the ſameneſs or diverſity of the ſaltneſs of the ſea in different places, were made in the courſe of the voyage towards the North Pole, in 1773. We learn from theſe experiments, that the ſea water at the *Nore* contained not quite $\frac{1}{36}$ of ſalt; at the back of *Yarmouth* ſands not quite $\frac{1}{32}$; off *Flamborough Head* rather more than $\frac{1}{79}$; off *Shetland* rather leſs than $\frac{1}{29}$; latitude 74 at ſea $\frac{1}{29}$; latitude 78 leſs than $\frac{1}{28}$; latitude 80, near the ice, not quite $\frac{1}{30}$; latitude 80. 30, under the ice, not quite $\frac{1}{28}$; latitude 68.46, rather more than $\frac{1}{28}$; latitude 65, at ſea, rather leſs than $\frac{1}{28}$*. "Dr.

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* Theſe ſeveral quantities are derived from the experiments mentioned in a Voyage towards

Hales got only $\frac{1}{27}$ from water taken up in the *Mediterranean*, and $\frac{1}{29}$ from water taken up at the *Nore*. Dr. Rutty ſays, he got $\frac{1}{25}$ from water taken up in latitude 65; $\frac{1}{28}$ from water taken up near to *Dublin*; and $\frac{1}{30}$ from water taken up at *Dungarvan*; and Dr. Lucas, that he got $\frac{1}{25}$ from ſea water taken up at *Harwich* *." The reader will obſerve that theſe experiments do not perfectly accord, as to the quantity of ſalt contained in water taken up at the ſame place, as at the Nore, and in latitude 65. This is not to be wondered at; the ſea water in the ſame latitude, is not always equally di-

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wards the North Pole, 1773, by Conſ. John Phipps, now Lord Mulgrave, p. 143.

* Monro on Mineral Waters, Vol. I. p. 105.

diluted by the freſh water which falls from the atmoſphere; nor can we be certain, that different perſons, in making experiments of this kind, have always uſed the ſame degree of heat, in drying the reſidue which remains after the evaporation of the water; or that they have evaporated the water with the ſame degree of heat: and yet this circumſtance alone may occaſion a great diſparity in the quantity of the ſaline matter, ſeparable from a definite weight of ſea water. For not to inſiſt upon, what has been before mentioned, the decompoſition of a part of the ſea ſalt, which takes place when a ſolution of it is evaporated with a great heat, it may be obſerved, that the ſea ſalt in ſubſtance is diſperſed, when a ſaturated ſolution of it is evaporated with even leſs than a boil-a boiling heat. This is very clearly ſeen, when a ſmall quantity of a ſolution of ſea ſalt is evaporated to dryneſs in a deep copper veſſel, for the inſide of the veſſel, which can come in contact with nothing but the vapour, is ſpangled with particles of ſalt, which have been carried up with the vapour.

It appears from ſome experiments, formerly made in a voyage from England to *Bombay* in the Eaſt Indies, that the weight of the ſea water was the greateſt, not preciſely at the Equator, but where the ſun was vertical, and conſequently, in ſimilar circumſtances, where the heat was greateſt. The greateſt weight of a definite quantity of ſea water, which was obſerved in ſailing from 28 degrees, north latitude, to the Cape of *Good Hope*,

Hope, which is about 34½ degrees ſouth latitude, in the months of May, June, and July, was at St. Jago iſland, north latitude, 15 degrees; and the leaſt was at Tenerif iſland, north latitude 28. The weights of equal bulks of Thames water, of the ſea water at Tenerif, and St. Jago, were 659—673½—780½ grains *; the proportion of which numbers may be nearly thus expreſſed.—Thames water 1000,....Tenerif ſea water 1022,—St. Jago ſea water 1184. By comparing theſe numbers with a Table in the Philoſophical Tranſactions, exhibiting the comparative weight of equal bulks of ſimple water, and of water impregnated with different portions of ſea ſalt, it may be conjectured, that the Tenerif ſea water

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* Gentleman's Mag. Vol. XXV. p. 260.

contained about $\frac{1}{32}$ of ſalt, and the St Jago water above $\frac{1}{4}$ of its weigh of ſalt, ſo that it was nearly, if no fully, ſaturated*.

As it is not every perſon who ca make himſelf expert in the uſe of the common means, of eſtimating th quantity of ſalt contained in ſea water, I will mention a method of doing it, which is ſo eaſy and ſimple that every common ſailor may underſtand and practiſe it; and which, a the ſame time, from the trials I hav made of it, ſeems to be as exact method, as any that has yet bee thought of.—Take a clean towel o

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* I formed the table here referred to abo 10 years ago; I have reaſon to think, th thoſe who have leiſure and accurate inſtr ments, might make it more correct, by uſi more precaution than I did in ſome particula eſpecially in what relates to the purity of t ſalt. Philoſ. Tranſ. 1770, p. 349.

any other piece of cloth, dry it well in the ſun or before the fire, then weigh it accurately, and note down its weight; dip it in the ſea water, and, when taken out, wring it a little till it will not drip, when hung up to dry; weigh it in this wet ſtate, then dry it either in the ſun or at the fire, and, when it is perfectly dry, weigh it again. The exceſs of the weight of the wetted cloth above its original weight, is the weight of the ſea water imbibed by the cloth; and the exceſs of the weight of the cloth, after being dried, above its original weight, is the weight of the ſalt retained by the cloth; and by comparing this weight, with the weight of the ſea water imbibed by the cloth, we obtain the proportion of ſalt contained in that ſpecies of ſea water.

Whoever undertakes to aſcertain the quantity of ſalt, contained in ſea water, either by this or any other method, would do well to obſerve the ſtate of the weather, preceding the time when the ſea water is taken out of the ſea, for the quantity of ſalt contained in the water near the ſurface, may be influenced, both by the antecedent moiſture, and the antecedent heat of the atmoſphere. And this leads to the conſideration of a queſtion propoſed by *Ariſtotle*[*], —Why are the upper parts of the ſea ſalter and warmer than the lower? .....Some philoſophers admitting the fact, have followed him in attempting to explain it; whilſt others have thought themſelves authorized by experiment, to deny the truth of the poſition; and thoſe, perhaps, will argue with the greateſt juſtneſs, who ſhall affirm, that it is neither generally to be admitted, nor generally to be rejected, but that the ſea in ſome places, and under certain circumſtances, is ſalter and warmer at the ſurface, than at any conſiderable depth beneath it, while in many others the reverſe is true. The queſtion conſiſts of two parts, betwixt which, though there, probably, is a connection, yet it is not ſo neceſſary a one, as to hinder us from conſidering each part by itſelf.

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[*] Ariſt. Prob. S. xxiii. 9. 30.

As to the ſaltneſs of the ſea at different depths, we have different accounts concerning it. There are two methods of eſtimating the relative quantities of ſalt contained in different ſpecimens of ſea water; one conſiſts in weighing equal bulks of them, ſuppoſe a pint or a bottle full of each; for if they have equal weights, it is ſafely enough inferred, that they contain equal quantities of ſalt: the other conſiſts in evaporating a pint, or any other meaſure of ſea water, and weighing the ſalt (which is not evaporable) remaining after the water is wholly diſſipated. Mr. Boyle found, that equal bulks of water taken up in the *Channel* between *France* and *England* at the ſurface, and at the depth of fifteen fathoms, were equally heavy, and thence he concludes, that the ſuperficial water was as ſalt as that at a great depth.*

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* Boyle's Obſerv. on the Saltneſs of the Sea.

On the other hand, Count Marſilli obtained by evaporating the water, a thirty-ſecond part only of its weight of ſalt from water, taken up on the ſurface of the ſea in the gulf of *Lyons*, whilſt that taken up at the ſame time and place from a great depth gave him a twenty-ninth part of its weight of ſalt; and he thence infers, the greater ſaltneſs of the ſea at the bottom than at its ſurface*. In this concluſion he has been ſupported by experiments made in the gulph of *Bothnia*.

There can be no reaſon to ſuſpect the accuracy with which any of theſe experiments was made: in different places the facts will probably be different. The *Rhone* diſcharges much *freſh* water into the gulf of *Lyons*;

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* Hiſtoire Phyſ. de la Mer.

Lyons; this freſh water being lighter than the ſea water, will not readily mix itſelf with it, and for this reaſon the ſuperficial water will be moſt diluted by it, and from that circumſtance it will contain leſs ſalt, in a definite quantity, than the water which is at a greater depth. At Northwich they pump their brine, which is to be boiled into ſalt, into a very large circular baſon, ſunk in the ground, and lined with brick, from whence, when its impurities are ſubſided, it is conveyed to the boiling pans. This baſon is expoſed to the open air, and in caſe of a great fall of rain, or ſnow, they let off the uppermoſt parts of the water through a wooden tube, which is at other times ſtopped with a plug, placed upon a level, nearly, with the upper ſurface of the brine in the baſon; what is thus let off is not preſerved, but thrown away as uſeleſs. This practice proceeds upon the principle we are ſpeaking of, namely, that freſh water is not apt to mix itſelf, unleſs it be much agitated with ſalt water. The gulf of *Bothnia* receives rivers from an immenſe range of mountains, extending themſelves to the eaſt, the north, and the weſt, far beyond the polar circle; and therefore, as in the gulf of Lyons, its ſurface may be more diluted with freſh water than its middle or bottom. But in the water of the *Engliſh channel*, with which Mr. Boyle made his experiment, the caſe is very different. This water has a communication with the German ocean on one ſide, and with the Atlantic on the other; and, from that circumſtance, it is ſubject to ſuch a conſtant agitation as will occaſion all the freſh water, poured into it by the Thames, Medway, and other rivers of leſs conſequence, to be uniformly diffuſed through all its ſubſtance, ſo as to render it equally ſalt at all depths. The conſtant effect produced on the ſuperficial water of the ſea, by the influx of a large river, is ſomewhat ſimilar to the temporary effect, attending a downfal of rain; the ſuperficial water will be thereby much diluted. It was obſerved in the voyage to Bombay before referred to, that if a quantity of ſea water in dry weather weighed 778 grains, an equal quantity immediately after a fall of rain would not weigh above 676 grains, the ſame quantity of Thames water weighing 659 grains. Hence it may be collected, that the weight of a cubic foot of the ſea water before rain exceeded the weight of a cubic foot after rain by 155 ounces.

In the voyage which was made towards the North Pole in 1773, we meet with ſome experiments on this ſubject.

In the open ſea off Shetland, north latitude 60 degrees, equal quantities of water were taken up at the ſurface, and at the depth of ſixty-five fathoms; theſe different waters yielded by evaporation equal quantities of ſalt, namely, $\frac{1}{29}$ of their weight nearly.

On another occaſion, in north latitude 65 degrees, ſome water taken up from the ſurface gave nearly $\frac{1}{28}$ of its weight of ſalt, whilſt an equal weight taken up from the depth of 683 fathoms in the ſame place, gave only $\frac{1}{32}$ *.

It is obvious that the firſt of theſe experiments confirms Mr. Boyle's notion of the ſea being equally ſalt at all depths, and that the ſecond is wholly oppoſite, both to his opinion, and that of Count Marſilli, ſo that the queſtion remains ſtill undecided. There is a circumſtance, however, which I will ſubmit to the reader's con-

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* In page 143, from which this account is extracted, the latitude is put equal to 75 degrees; but it appears from pages 117 and 137, that the ſhip on the 4th of Sept. when this experiment was made, was in latitude 65.

conſideration, whether it may not be ſufficient to explain the different reſults of the experiments made during the voyage here ſpoken of. The experiments which gave equal quantities of ſalt from equal quantities of water, taken up at the ſurface, and at the depth of 65 fathoms, were made on the 12th of June; the others, which gave more ſalt from the ſuperficial, than from the deep water, were made on the 4th of September. Now admitting the ſaltneſs of the ſuperficial water, and of water at the depth of 683 fathoms to have been the ſame, in the beginning of June, why may not the quantity of water, evaporated from the ſurface of the ſea, during the hot months of July and Auguſt, have left the ſuperficial water more impregnated with ſalt than the water at a great depth? It is ſome confirmation of this notion, that the air in June was only ſix degrees warmer than the water from the depth of 65 fathoms, and that the air in September, when the experiment was made, was above 26 degrees hotter than the water from the depth of 683 fathoms. The riſe of freſh ſprings at the bottom of the ſea, in particular places, may be another reaſon for our accidentally finding the ſea ſalter at its ſurface than at any great depth below it. However, neither of the reaſons here offered in explanation of the phænomenon are wholly to be relied on; for the freſh water which may accidentally be found at the bottom of the ſea will riſe up till it becomes as much impregnated with ſalt, as the water incumbent over it is, and the water at the ſurface, which is rendered heavier by the ſun's having evaporated ſome of its freſh particles, will probably ſink downwards, and mix itſelf with the water below it, till the whole maſs of water becomes equally heavy, and conſequently equally ſalt. The fact itſelf, of the ſea being in any caſe ſalter at the ſurface than at its bottom, ought to be aſcertained by repeated experiments.

With reſpect to the temperature of the ſea at different depths, it ſeems reaſonable enough to ſuppoſe, that in ſummer time it will be hotter at the ſurface than at any conſiderable depth below it, and that in winter it will be colder. Suppoſe a ciſtern, twelve feet in depth, to be filled with ſpring water, of 48 degrees warmth, to the height of eleven feet; then if we fill up the ciſtern to its top, by gently pouring water heated to 100 degrees upon the ſurface of the ſpring water, it may readily be underſtood, that the heat of this water will not be inſtantaneouſly communicated through the whole maſs of water in the ciſtern, but that the water will decreaſe in heat from the ſurface to the bottom of the ciſtern. On the other hand, if on the 11 feet of ſpring water heated to 48 degrees, we pour a foot of water heated only to 33 degrees, it may be expected, that the ſpring water which is neareſt to the cold water, will be ſooner cooled by it than that which is at a greater diſtance; and on this account the water at the bottom of the ciſtern vill be warmer than that in the niddle or at the top. It muſt be bſerved, however, that cold water eing, bulk for bulk, heavier than ot water, the water which has only 3 degrees of heat will deſcend by s ſuperior weight, into the maſs of ater contained in the ciſtern, and us the water in the ciſtern will be ooled, not only by the bare comunication of cold from the upper ater, but by the actual mixture of at water with the reſt, ſo that the ifference between the heat of the ater at the bottom and top, will ot be ſo great as it would have een, if the cold water had not ixed itſelf with the reſt. Theſe

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ſuppoſitions of hot and cold water incumbent on the ſpring water in the ciſtern, are analogous to the action of the ſummer and winter atmoſpheres incumbent on the ſurface of the ſea. No perſon, who has bathed in deep ſtanding water in ſummer time, can have failed to obſerve, that the water grew colder and colder, according to the depth to which he deſcended. I have frequently obſerved, that the ſurface of a pool of water of two feet in depth, has, in a ſunny day, even in winter, been five degrees hotter than the water at its bottom.

Mr. *Wales* deſcribes the inſtrument he made uſe of for trying the temperature of the ſea at different depths, in the following terms:

“ The apparatus for trying the heat of the ſea water at different depths, conſiſted of a ſquare wooden tube, of about 18 inches long, and 3 inches ſquare externally. It was fitted with a valve at the bottom, and another at the top, and had a contrivance for ſuſpending the thermometer exactly in the middle of it. When it was uſed, it was faſtened to the deep-ſea line, juſt above the lead, ſo that all the way as it deſcended the water had a free paſſage through it, by means of the valves which were then both open; but the inſtant it began to be drawn up, both the valves cloſed by the preſſure of the water, and of courſe the thermometer was brought up in a body of water, of the ſame temperature with that it was let down to*." With this inſtrument, which is much the ſame with one formerly deſcribed by Mr. Boyle, in his obſervations about the ſaltneſs of the ſea, water was fetched up from different depths, and its temperature accurately noticed, in different ſeaſons and latitudes.

Auguſt 27, 1772, ſouth latitude 24°. 40′. The heat of the air was 72½, — of the water at the ſurface 70,—of water from the depth of 80 fathoms 68 †.

December 27, 1772, ſouth latitude 58° 21′. The heat of the air was 31½,—of the water at the ſurface 32, — of water from the depth of 160 fathoms $33\frac{1}{2}$*.

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* See Aſtronomical Obſervations made in a Voyage towards the South Pole, &c. in 1772, 1773, &c. by W. Wales. Introduction, p. 53.

† Wales' Obſer. p. 206.

In the voyage to the high northern latitudes before mentioned, they made uſe of a bottle to bring up water from the bottom, which is thus deſcribed. "The bottle had a coating of wool, three inches thick, which was wrapped up in an oiled ſkin, and let into a leather purſe, and the whole incloſed in a well-pitched canvas bag, firmly tied to the mouth of the bottle, ſo that not a drop of water could penetrate to its ſurface. A bit of lead ſhaped like a cone, with its baſe downwards, and a cord fixed to its ſmall end, was put into the bottle; and a piece of valve leather, with half a dozen ſlips of thin bladder were

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* Wales' Obſer. p. 208.

ſtrung on the cord, which, when pulled, effectually corked the bottle on the inſide." I have here put down two of the experiments which were made during that voyage.

Auguſt 4, 1773, north latitude 80°. 30′. The heat of the air was 32,—of the water at the ſurface 36,—of water fetched up from the depth of 60 fathoms under the ice 39*.

September 4, 1773, north latitude 65°. The heat of the air was 66½,—of the water at the ſurface 55,—of water from the depth of 683 fathoms 40.

It appears from all theſe experiments that, when the atmoſphere was hotter than the ſurface of the ſea, the ſuperficial water was hotter than that at a great depth; and when the atmoſphere was colder than the ſurface of the ſea, it is evident that the ſuperficial water was ſomewhat colder than that at a conſiderable diſtance below it: and I doubt not that this will generally be the caſe, though ſudden changes in the temperature of the atmoſphere, which cannot be inſtantly communicated to the ſea, may occaſion particular exceptions.

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* Voyage towards the North Pole, p. 143.

In the year 1779, ſeveral experiments were made, with great accuracy, in order to inveſtigate the temperature of the lake of *Geneva*, and of other lakes in Switzerland, at different depths; we learn from them, that in winter time there is very little difference between the heat of the water at the ſurface, and at a great depth below it; but that in ſummer, the ſuperficial water is conſiderably warmer than that which is at a great diſtance from the ſurface. The experiments were made with a thermometer graduated after Reaumer's ſcale; ſome of them, reduced to Fahrenheit's ſcale, are expreſſed below.

Temperature of the lake of *Geneva* at different depths, in the beginning of February 1779, after a month's uninterrupted froſt.

Heat of the open air variable from 37 to 40 degrees.

In

Water at the ſurface of the lake,	$42\frac{1}{8}$
Depth 100 feet - - -	$42\frac{1}{8}$
Depth 250 feet - - -	$42\frac{1}{8}$
Depth 950 feet, bottom -	$41\frac{27}{70}$

In another part of the lake, open air from 37 to 40.

Surface - - - - -	$42\frac{1}{8}$
Depth 350 feet - - -	$42\frac{1}{8}$
Depth 620 feet, bottom - -	$41\frac{27}{80}$

Temperature of the lake of *Neuchâtel*, July 17, 1779.

Air - - - -	$75\frac{1}{5}$
Surface - - - -	$73\frac{5}{8}$
Depth 225 feet, bottom - -	41*

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* The reader will find a great many other experiments of the ſame nature, in the firſt volume (the only one yet publiſhed) of a very intereſting work, intitled, Voyages dans les Alpes, par Horace-Benedict de Sauſſure, Profeſſor of Philoſophy at Geneva.

ESSAY

ESSAY V.

OF FRESH WATER PROCURABLE FROM SEA WATER, BY CONGELATION, AND BY DISTILLATION.

"THE ſecond day we deſcried a main bank of ice, in the latitude of 60°. We wanting freſh water did ſail cloſe to this land of ice, and hoiſted out our boat and loaded her twice with ice, which made us very good freſh water*."—"About nine of the clock in the forenoon we came by a great iſland of ice, and by this iſland we found ſome pieces of ice broken off from the ſaid iſland, and being in great want of freſh water, we hoiſted out our boats of both ſhips, and loaded them twice with ice, which made us very good freſh water[*].”— “The pieces of ice we took up were hard and ſolid as a rock; the ſalt water which adhered to the ice, was ſo trifling as not to be taſted, and after it had lain on deck a little time, intirely drained off; and the water which the ice yielded was perfectly ſweet and well taſted[†].”—“In the afternoon we filled our caſks with freſh water, from the ice, which we found very pure and ſoft*."

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* Capt. Weymouth's Voy. in 1602. Purchaſs' Col. B. iv. c. 13.

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[*] Capt. Weymouth's Voy. in 1602. Purchaſs' Col. B. iv. c. 13.

[†] Cook's Voy. towards the S. Pole in 1773, Vol. I. p. 37.

Notwithſtanding theſe teſtimonies of our moſt experienced navigators, concerning the freſh water which they procured from the thawing of the ice, they found floating in the ſea; yet it might ſtill remain a matter of doubt, whether the ice, from which the water was obtained, had been formed in the ſea, and, conſequently, whether ſea water itſelf would when frozen yield freſh water. For it might, with ſome appearance of probability, be ſaid, that the ice had either been formed at the mouths of large freſh water rivers, and thence by tides or torrents drifted into the ſea; or that it had been broken by its own weight from the immenſe clifts of ice and frozen ſnow, which, in countries where there are few rivers, are found in high latitudes to project a great way into the ſea*; or laſtly, admitting it to be formed in the ſea, it might be urged, that it was formed of freſh water, not of ſalt. *Fotherbye*, in his voyage in 1614, eſteems ſnow the original cauſe of the ice found at ſea, for he obſerved it to be an inch thick upon the ſurface of the ſea; and Captain Cook, from his own obſervations in the South

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* Phipps' Voy. towards the N. Pole in 1773, p. 60.

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* *Icebergs* are large bodies of ice filling the vallies between the high mountains. — Large pieces frequently break off from the icebergs, and fall with great noiſe into the water: we obſerved one piece which had floated out into the bay, and grounded in twenty-four fathom; it was fifty feet high above the ſurface of the water. Phipps' Voy. p. 70.

Sea, was diſpoſed to think, that the vaſt floats of ice he met with in the ſpring, were formed from the congelation of ſnow *. Without doubt, the ſnow which falls upon the ſurface of the ſea, being in a ſolid ſtate, and, bulk for bulk, lighter than the ſea water, will not readily mix itſelf with it; but may, by a due degree of cold in the atmoſphere, be ſpeedily converted into a layer of ice, the upper ſurface of this firſt layer of ice being elevated above the ſurface of the ſea, will receive all the freſh water which falls from the atmoſphere in the form of ſnow, ſleet, rain, or dew; by the ſucceſſive congelation of which, the largeſt fields of ice may at laſt be formed. Whether the ice found at ſea,

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* Voy. towards the S. Pole, Vol. II. p. 242.

ſea, be formed according to any one or all of theſe ways, it is evident, that it is produced from an accumulation of congealed freſh water, and that we cannot, from the freſh water procurable from ice thus formed, conclude that the ice of frozen ſea water would yield freſh water.

To a navigator, it is a matter of little conſequence to determine, whence the ice which ſupplies him with freſh water is produced, he is ſure of meeting with more than enough of it for his purpoſe, and he leaves it to the leiſure of philoſophers to decide the queſtion, whether congealed ſea water will, when thawed, yield freſh water. They have decided it by actual experiments, made with every ſuitable attention. Some ſea water was taken up off the *North Foreland*, it was expoſed to a freezing atmoſphere, and it afforded an ice perfectly free from any taſte of ſalt. The ſpecific gravity of the water, produced from the melting of the ice, was ſomewhat greater than that of diſtilled rain water, and ſomewhat leſs than that of a mixture of rain and ſnow water taken out of a water tub. The degree of cold, in which the ſea water froze, was 28½ of Fahrenheit's thermometer, or 3½ lower than that in which common water freezes*. The degree of cold, however, in which ſea water freezes, cannot be aſcertained in general, unleſs it can be ſhewn, that the ſea water, taken up in different latitudes, contains equal quantities of ſalt, the contrary of which is probably true. And in

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* Philoſ. Tranſ. for 1776, p. 251. & 373.

general, the greater the quantity of ſalt contained in any portion of ſea water, the greater will be the degree of cold neceſſary to freeze it.

We had a ſharp froſt at Cambridge on the 21ſt of laſt December; I filled a thin phial, holding 8 ounces, with ſea water, which contained $\frac{1}{32}$ of its weight of ſalt, and expoſed it ſuſpended from the branch of a tree, during the night of the day before mentioned, to the cold of the atmoſphere. In the morning I found that a part of the water had been frozen: I poured the unfrozen part from the other, and having rinſed the ice in freſh water, ſo as to ſeparate from it the ſalt water which adhered to it, I thawed it before the fire, and found the water which it gave to be wholly free from ſalt.

It

It had been long ago aſſerted by Macrobius *, that ſea water was never frozen, but that the ice found at ſea, proceeded from the freezing of the freſh water ſpread over its ſurface by the influx of rivers. And in modern times, the opinion, concerning the freezing of ſea water, ſeems to have been admitted with great caution, by our moſt experienced navigators. They were aware, indeed, that the Baltic and other large ſeas, were frequently frozen quite over, but they appear to have thought that falls of ſnow laid a ſufficient foundation for the production of that event, "without its being at all neceſſary for the ſea water to freeze ‡." I cannot conceive that there was any

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* Macrob. Sat. L. vii. c. 12.

‡ Cook's Voy. Vol. II. p. 242.

great occaſion for this cautious mode of expreſſion, ſince it had been obſerved, above a hundred years ago, that not only ſea water, but water containing double the proportion of ſalt commonly found in our ſea water, and more than is contained in the ſea water of any climate, might be frozen by the cold prevailing in our atmoſphere *.

This freezing of ſea water, moreover, was formerly practiſed, and, I believe, is ſtill practiſed in ſeveral parts of the north of Europe, with a view to leſſen the expence and trouble of extracting ſalt from ſea water †. Stahl informs us of a chemiſt, who, near

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* Boyle's Works, fol. ed. Vol. II. p. 264.

† Junck. Conſ. Chem. Tab. xviii. ſ. 15. And Wallerii Mineral. French Tranſ. Vol. I. p. 316.

near a century ago, endeavoured to expedite the buſineſs of making ſalt in Sweden and Livonia, by means of froſt. With this view he propoſed the making large reſervoirs to contain the ſea water, and the pumping the concentrated brine from under the ice into proper veſſels, to be boiled in the ordinary way. He obtained a patent, but a combination of ſalt-boilers ruined his project*.

I cannot ſee why this cuſtom might not be introduced into England with great advantage. A third part at leaſt of ſea water may, by freezing, be converted into ice by the ordinary cold of our winters: this ice, conſiſting entirely of freſh water, may be thrown away as uſeleſs, and the remaining

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* Traité des Sels, par M. Stahl. French Tranſ. 1771, p. 143.

maining two thirds, being boiled down, will give all the ſalt contained in the whole of the ſea water; thus there would be a ſaving at leaſt of one third part of the fuel employed in boiling ſea water into ſalt *. When it is ſaid, that the unfrozen ſea water contains all the ſalt, this muſt be underſtood with ſome reſtriction, ſince it is probable, that the frozen part will, upon moſt occaſions, hold a little ſalt, but not ſo much as is contained

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* Briſtol and other waters containing earth, are found to depoſit their earth, or a great portion of it, by being frozen.—Brownrigg on Salt, p. 76. Some ice, taken from under the ſurface of the ſea in *Hudſon*'s Streights, being melted, gave 6 ounces of ſalt from 40 quarts of water.—It is probable, that it would not have given ſo large a proportion of ſalt, had it been previouſly cleanſed from the adhering ſea water. Philoſ. Tranſ. 1740, p. 807.

tained in many ſpring waters, which are reckoned ſufficiently pure. Eight pounds of frozen ſea water, taken up near Calais, left upon evaporation 32 grains of reſiduum, about half of which was ſea ſalt. The ſame quantity of unfrozen ſea water, taken up at the ſame place after the froſt, left by evaporation 1412 grains of reſiduum, ſo that the water, by being frozen, had freed itſelf from 1380 grains of heterogeneous matter *.

The ice found at ſea is of different colours and conſiſtencies, ſome is poliſhed and pellucid, and thought to be freſh water ice; another kind is of a ſpungy texture, and of a pale green colour like vitriol; and it has been affirmed, that this kind of ice is

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* Journ. des Scavans, Mai, 1769. See, alſo Boerh. Chem. Vol. I. p. 721.

is ſalt, and conſiſts of ſalt water frozen *. This is a miſtake, both as to the fact and the reaſon of it; for the vitriol coloured ice, when freed from the ſalt water in which it ſwims, is quite freſh †, and ſea water when frozen, we have ſeen, does not yield a ſalt ice.

We may, perhaps, be leſs ſurpriſed at the particles of water, in which ſea ſalt is diſſolved, ſeparating themſelves from the particles of the ſalt, and coaleſcing together without admitting any junction with ſuch a heavy extraneous body, when we conſider, that during the action of freezing, they refuſe all union even with the air itſelf. Water in its fluid ſtate

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* Crantz's Hiſt. of Greenland, B. I. c. ii. ſ. 13.

† Philoſ. Tranſ. for 1770, p. 111.

ſtate is ſaturated with air, but it is in a great meaſure, if not wholly, deprived of it by being frozen; and modern philoſophers have followed the opinion of Ariſtotle, in attributing the inſalubrity (real or ſuppoſed) of water which has been frozen, to the loſs of its air *. "The water we melted out of the ice (taken up at ſea) was perfectly freſh, and had a purer taſte than any we had on board. If any fault could be found with it, it was that the fixed air was ex-

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* Non ſolo rigore nivalis aqua pernicioſa eſt, ſed ob aliam cauſam quam non pigebit aperire auctore Ariſtotele. Omnis aqua, inquit, habet in ſe aeris tenuiſſimi portionem quæ ſalutaris eſt —nix ergo, quæ nihil aliud eſt quàm aqua in aere denſata, tenuitatem ſui cum denſaretur amiſit, et ideo ex ejus reſolutæ potu diverſa morborum genera viſceribus inſeminantur. Macrob. Sat.

expelled from it, by which means almoſt every one who uſed it, was affected with ſwellings in the glands of the throat. Water melted from ſnow or ice is known always to have this effect, and the conſtant uſe of it in mountainous countries produces thoſe enormous wens which are common among Alpine nations *". If the loſs of the water's air be the true cauſe of the tumid throat, obſervable among the inhabitants of the Alps, and of the glandular ſwellings here mentioned, it ſeems as if a very ſimple proceſs would free it from this noxious quality, and reſtore it to its original ſalubrity. A ſimple expoſure to the air, for a few days, would be ſufficient for the purpoſe; ſince water, which has been

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* Forſter's Voy. Vol. I. p. 107.

been deprived of its air, imbibes it afreſh very readily, and becomes ſaturated with it in no great length of time. And this readineſs of imbibing air makes a material difference between the water obtained from the ſimple thawing of ſnow, and river water, though the river water itſelf ſhould proceed from melted ſnow; for being in its courſe conſtantly expoſed to the air, it will ſoon regain the air which it had loſt by being frozen.

This may not be an improper place to obſerve, that ſeveral phyſicians have rejected the notion of the uſe of ſnow water being the cauſe of the wens here ſpoken of, and that upon good grounds. For in *Greenland*, where ſnow water is commonly uſed, theſe unſightly protuberances are never met with *; nor did I ever ſee one of them in *Weſtmoreland*, where we have higher mountains, and more ſnow than they have in *Derbyſhire*, in which country they are very common. But what puts the matter beyond doubt is, that theſe wens are common in *Sumatra*, where there is no ſnow during any part of the year †.

The poſſibility of extracting freſh water from ſea water by diſtillation, has been long known; and indeed it ſeems as if it could never have been queſtioned by thoſe, who duly reflected upon the origin of all the freſh water we meet with on the ſurface of the earth. The geography of the globe is yet but imperfectly known; enough, however, is known of it to convince us that a much larger portion of its ſurface is occupied by ſea than by land. The *Atlantic* Ocean appears at firſt view to be larger than the whole Weſtern Continent, and the area of the reſt of the habitable world, even if we take into conſideration the land recently explored in the ſouthern hemiſphere, ſeems as if it could not equal half that immenſe expanſion of water which conſtitutes the *Indian* and *Pacific* Oceans. Philoſophers have attempted by meaſuring the moſt approved maps, and by other ingenious methods, to aſcertain the exact proportion between the land and ſea; great preciſion cannot reaſonably be expected to accompany this inquiry; but when we learn from one, that the ſea covers near three fourth parts *, and from another, that it covers four fifths † of the ſurface of the globe, we may ſurely reſt ſatisfied of the truth of the general poſition, that there is a much greater extent of ſea than of dry land. This immenſe maſs of ſalt water which ſurrounds the globe, is the great primary ſource from which all freſh water is derived. The vapour which is inceſſantly elevated from thence by the heat of the ſun, the action of the wind, and other

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* Rutty's Synop. p. 42.

† Philoſ. Tranſ. for 1778, p. 165.

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* See Dr. Long's Aſtron. Vol. I. p. 168.

† The ſeas, and unknown parts of the earth (by a meaſurement of the beſt maps) contain 160,522,026 ſquare miles; the inhabited parts 38,990,569: Europe, 4,456,065; Aſia, 10,768,829: Africa, 9,564,807; and America, 14,110,874. Ferguſon's Tables, p. 300.

other leſs obvious cauſes, conſiſts, generally ſpeaking, of freſh water, which being diſcharged from the atmoſphere in the form of dew, rain, hail, or ſnow, conſtitutes ſprings, rivers, and ſtagnant pools. The taſte of ſea water is, we know, not only ſalt but bitter; the *common ſalt*, which occaſions its ſaltneſs, and the *Epſom ſalt*, which occaſions its bitterneſs, can neither of them be raiſed in vapour (in any ſenſible proportion) by the ſame gentle degree of heat with which water may be raiſed; in order, therefore, to obtain freſh water from ſea water, we need only copy the proceſs of nature, and diſtil ſea water with a gentle heat. This method of obtaining freſh water from the diſtillation of ſea water, was practiſed by Sir R.

Hawkins, in the reign of Queen Elizabeth:—“ Although our freſh water had failed us many days, yet with an invention I had in my ſhip, I eaſily drew out of the water of the ſea a ſufficient quantity of freſh water, to ſuſtain my people, with little expence of fuel; for with four billets, I ſtilled a hogſhead of water, and therewith dreſſed meat for the ſick and whole. The water ſo diſtilled was found to be wholeſome and nouriſhing*.”

Since the time of Sir R. Hawkins, a great variety of attempts have been made in our own, and in other countries, to procure freſh water from ſea water; they have in general been little elſe than different modes of diſtillation, but many of the authors of them, either from an ignorance of the real nature of ſea water, or from a deſign of rendering their proceſs myſterious, have mixed different ingredients with the water either before or after its diſtillation. The manner of procuring freſh water, by the ſimple diſtillation of ſea water, ſeems at preſent to have attained a great degree of perfection, both in *France* and *England*. M. de *Bougainville*, in his Voyage round the World, has borne honourable teſtimony to the utility of the machine for diſtilling ſea water, which was made public in 1763, by M. *Poiſſonnier*, its inventor. Lord *Mulgrave*, in his voyage towards the North Pole, in 1773, has done equal juſtice to the method of obtaining freſh water from the ſea

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* Purchaſs' Collec. of Voy. B. vii. c. 5.

by diſtillation, which was introduced into the Engliſh navy, in the year 1770, by Dr. *Irving*. Whether the invention of M. Poiſſonnier ſuggeſted any hints to Dr. Irving, is beſt known to himſelf; but he has unqueſtionably added ſuch *improvements* to that method (to ſay no more), as ſeem to have juſtly intitled him to the parliamentary reward of five thouſand pounds, which he has obtained. The French are as jealous of the glory of their nation in arts as in arms, and the Engliſh, we truſt, will never be their inferior in either; it is to this exceſs of jealouſy we muſt attribute the acerbity, with which one of the beſt chemiſts of the age ſpeaks of Dr. Irving, as having deceived the Parliament of England, by appropriating to himſelf the diſcovery of M. Poiſſonnier*.

In order that the reader may have a clear notion of Dr. Irving's method, let us ſuppoſe a tea-kettle to be made without a ſpout, and with a hole in the lid, in the place of the knob: then the kettle being filled with ſea water, the *freſh* vapour which ariſes from the ſea water as it boils, will iſſue out through the hole in the lid; into that hole fit the mouth of a tobacco-pipe, letting the ſtem have a little inclination downwards, then will the vapour of freſh water take its courſe through the ſtem of the tube, and may be collected by fitting a proper veſſel to its end. This would be an apt repreſentation of Dr. Irving's con-

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* Baumé's Chy. Vol. III. p. 588.

trivance: he has adapted a tin tube, of ſuitable dimenſions, to the lid of the common kettle uſed for boiling the proviſions on board a ſhip; the freſh vapour which ariſes from boiling ſea water in the kettle paſſes through this tube into a hogſhead, which ſerves as a receiver; in order that the vapour may be readily condenſed, the tube is kept cool, by being conſtantly wetted with a mop dipped in a tub of cold ſea water.

It appeared from the teſtimony which was delivered to the Lords of the Admiralty, by many reſpectable officers who were eye witneſſes to an experiment, made on board the *Arrogant*, at Spithead, in January 1771, that " 80 gallons of ſea water did, in twenty-five minutes, after being put into the Arrogant's copper, and a fire made, diſtil in the proportion of twenty-five gallons per hour, into freſh water, perfectly well taſted, and of leſs ſpecific gravity than the beſt ſpring water in that neighbourhood;" and the ſaid officers gave it as their opinion, that 500 gallons of freſh water might be diſtilled in the ſpace of 24 hours, with the ſame quantity of fuel in proportion to the time, as is required in the ordinary buſineſs of the ſhip. Every ſhip's kettle is divided into two parts by a partition in the middle, one of theſe parts is only in uſe when peaſe or oatmeal are dreſſed, but water is at the ſame time kept in the other to preſerve its bottom. Dr. Irving has availed himſelf of this circumſtance, and by filling the ſpare part of the copper with ſea water, and fitting on the lid and tube, he has ſhewn, that 60 gallons of freſh water may be drawn off, during the boiling of either of the abovementioned proviſions, without the uſe of any additional fuel: he recommends alſo the preſerving the water which may be diſtilled from the coppers in which peaſe, oatmeal, or pudding are dreſſing, as both a ſalutary beverage for the ſcorbutic, and the moſt proper kind of water for boiling of ſalt proviſions.

In November, 1771, *Lord Sandwich* ſent me two bottles of the beſt diſtilled water which could be prepared by Dr. Irving from ſea water, deſiring my obſervations upon it. In compliance with his Lordſhip's requeſt, I made the following experiments,

ments, of which I ſent him an account, without venturing to give the moſt diſtant opinion concerning the ſalubrity or inſalubrity of the water. — Exper. 1. Four ounces of the diſtilled ſea water being evaporated by a ſlow fire, upon a ſilver plate, left a pellicle, which was very ſenſibly ſaline to the taſte, and glutinous to the touch. — 2. Four ounces of the conduit water of Trinity college, treated in the ſame manner, left a pellicle, but not quite ſo large, nor ſo manifeſtly ſapid, as that from the diſtilled ſea water. — 3. The ſame quantity of common diſtilled water, being evaporated in the ſame way, left no ſenſible pellicle. — 4. The ſame quantity of common diſtilled water, impregnated with a ſmall drop of ſpirit of ſalt, left no pellicle upon the whole ſurface of the plate, but only a yellowiſh ſpot upon that part of it from which the laſt portion was evaporated; this ſpot was glutinous to the touch, and had a bitter metalline taſte.—5. The ſame quantity of common diſtilled water, impregnated with two grains of ſea ſalt, left a pellicle, ſimilar to that left by the diſtilled ſea water, except that it was ſomewhat whiter, had a leſs piquant taſte, and a leſs glutinous conſiſtency. From the 4th and 5th experiments, compared with the firſt, it may be inferred, that the diſtilled ſea water, which I examined, contained a ſmall portion of ſea ſalt, and alſo a ſmall portion of the acid of ſea ſalt uncombined with any baſis. A very minute portion of ſea ſalt, or of the acid of ſea ſalt, may be diſcovered in a large quantity of water, by the milkineſs which enſues on dropping into a glaſs full of the water, a drop or two of a ſaturated ſolution of ſilver in the acid of nitre.—6. I dropped a ſolution of ſilver in acid of nitre into common diſtilled water; no milkineſs enſued, nor was the tranſparency of the water changed. — 7. Into an equal quantity of the diſtilled ſea water, I dropped an equal number of drops of the ſolution of ſilver; the water became very turbid, and a copious white precipitation enſued. Theſe experiments ſhew, that the diſtilled ſea water is not wholly free from ſaline particles; but it probably contains them in ſo ſmall a proportion,

portion, as not to injure its ſalubrity in any ſenſible degree.

We may be the more diſpoſed to admit the poſſibility of diſtilled ſea water containing ſaline particles, when we reflect upon the ſmell of the acid of ſea ſalt, which circulates through the houſes in which ſalt is prepared, by boiling either ſea water or brine. The vapour which ariſes from the pans in which ſalt is boiled, being caught and condenſed in proper veſſels, and examined by proper teſts, gives indications both of ſea ſalt in ſubſtance, and of its acid. It is not an eaſy matter to aſcertain the preciſe degree of heat, in which ſaline particles begin to accompany the vapour which is raiſed from ſolutions of ſea ſalt; it is probably a degree far inferior to that in which water boils: at leaſt we are certain, that ſaline particles are found in rain and ſnow water, both of which are derived from vapours raiſed into the atmoſphere by the action of the ſun upon the ocean.

ESSAY VI.

OF CALCAREOUS EARTH, CRUDE AND CALCINED.

THE word *Calx* has been introduced into the Engliſh language from the Latin; it properly denotes the friable earth, to which ſome ſorts of bodies are reduced by fire; the operation by which they are reduced to that ſtate, is called *calcination*, and a very large claſs of bodies, from their being peculiarly ſubject to be converted by fire into a calx, have been denominated, by various mineralogiſts, *calcareous* bodies; the calx or powdery earth, procured from calcareous ſubſtances by calcination, is commonly called *lime*, more philoſophically *quicklime.*

Lead, iron, tin, and ſeveral other mineral ſubſtances, may reſpectively be reduced by fire to the ſtate of a calx; but the earths, obtainable from theſe ſubſtances by calcination, not poſſeſſing the other general qualities appertaining to the calces of calcareous ſubſtances, are not called quicklimes, but calces of this or that ſubſtance from which they are prepared.

In *Cambridgeſhire*, and many of the ſouthern counties of England, lime is prepared from the calcination of *chalk*, or, as it is here generally called, *clunch.* The kilns are inverted cones ſunk in the earth, and lined with brick; the baſe of the cone is about ten feet in diameter, and the depth of the kiln is about fourteen feet. One of theſe kilns will burn about 150 buſhels of lime in twenty-four hours; they uſe generally one buſhel of coal for every four buſhels of lime, and in ſummer, when the chalk is dry, they will ſometimes get five buſhels of lime, from the conſumption of one buſhel of coal; but coal being dear, the chalk is ſeldom well burned. In *Wales*, *Weſtmoreland*, *Derbyſhire*, and many other counties, they burn different ſorts of *limeſtone*, ſome of them approaching to the fineneſs of marble. In *Holland*, where they have neither chalk nor limeſtone, they drag fiſh *ſhells* out of the ſea with nets drawn by horſes, and burn the ſhells into lime. In the iſlands of the ſouthern hemiſphere they burn *coral rocks* into *chenam* or lime; and in ſome parts of *America* they calcine *madrepores*, and other ſpecies of coral for the ſame purpoſe. Lime may alſo be prepared from the calcination of all kinds of *marble*; of various kinds of *ſpar*; of *ſtalactites*, otherwiſe called *dropſtone*, and in Derbyſhire *watricle*; and of ſeveral other ſubſtances which conſiſt of *calcareous earth*, either *pure*, or mixed with *clay* or *ſand*, or other matters in different proportions. In *Cheſhire* and other countries, where they have no common materials for making o lime, it would be worth while to examine the earths which may be me with upon the ſurface of the ground or at a little diſtance below it; fo

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calcareous ſubſtances are not always united into hard compact maſſes, but are ſometimes found in the form of earth, and that of different colours.

Calcareous ſubſtances are in ſome countries calcined with pit-coal, in others with the cinder of pit-coal, in others with wood, in others with peat, in others with furze; the kind of fuel ſeems to be of no conſequence, provided the degree of heat be the ſame. Some ſorts, indeed, of pit-coal are apt, eſpecially when uſed in large quantities, to cake together, nd thus hindering, in ſome meaſure, he equal diffuſion of the heat, the ſtone remains unburned. In order that the heat may penetrate through very part of the ſubſtance to be calcined, it is uſual to break it into ſmall pieces; but if the calcareous ſtone happens to be naturally ſhaken or ſplit into layers, large pieces may be calcined, by placing them ſo that the flame of the fuel may, in aſcending, find its way between the ſeveral layers of which the ſtone conſiſts. In ſome parts of Yorkſhire, they burn pieces of this ſort of ſtone, a foot in thickneſs, and a foot and a half in length, without breaking them; they uſe generally eight dozen of coal to a kiln, and obtain twenty-two dozen of lime, the dozen containing thirty-ſix buſhels.

It is probable, that there is a certain definite degree of heat, to which any particular kind of calcareous ſubſtance ought to be expoſed, in order to its being converted into the beſt poſſible lime. If the heat be too ſmall, the whole of the ſubſtance will not be converted into lime; and on the other hand, it is poſſible, that the heat may be ſo great as to reduce the ſubſtance to a kind of glaſſy *ſcoria*; and whenever it becomes vitrified, it will loſe the properties of lime. I am aware of what is generally ſaid, that calcareous bodies, when pure, cannot be vitrified with the greateſt degree of heat; but though the heat of a chemical furnace may not be ſufficient for the purpoſe, yet there is reaſon to believe that a ſuperior degree of heat may effect a kind of vitrification; certain it is, that the lime burners are careful not to give too much heat, leſt they ſhould injure the quality of the lime. When the heat has been too violent, they, in many kilns, find ſeveral pieces of limeſtone covered with a glaſſy coat, and ſticking together. I do not take upon me abſolutely to ſay, that this effect is to be aſcribed to a beginning vitrification of the limeſtone; it may either proceed from thence, or it may be accounted for by ſaying, that the aſhes of the fuel adhering every where to the ſurface of the calcareous ſubſtance, have, in particular places, where they happen to be amaſſed in ſufficient quantity, contributed to its vitrification. This vitrification of calcareous earth, when mixed with other ſubſtances is obvious enough in the iron furnaces, where they are accuſtomed to mix limeſtone with the iron ore, the *ſcoria* or *ſlag* of an iron furnace perfectly reſembling glaſs.

There is a very ſingular experiment, which may be properly mentioned in this place. Mix well together equal weights of powdered chalk, or other calcareous body, and powdered pipe clay, put the mixture into a crucible *. Any where towards the middle of the crucible, put two ſolid pieces, one of pure chalk, the other of pure clay, ſo that they may not be contiguous to each other, but be every where ſeverally ſurrounded with the mixture of chalk and clay; then by expoſing the whole to a ſtrong degree of heat for a ſufficient length of time, it will be found that the mixture has been converted into

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* Crucibles are round or triangular veſſels of different dimenſions, wider at the top than at the bottom; they are principally uſed for the melting of metals, and are made of clay and ſand, or of clay and black lead, or of ſuch other materials as can reſiſt the ſtrongeſt fires without melting.

into glaſs; and upon breaking the vitrified maſs, we ſhall diſcover the two ſolid pieces of chalk and clay, unvitrified, and unaltered in their figure, though every where ſurrounded with glaſs. Any other ſort of clay would probably anſwer the purpoſe as well as pipe-clay, but I have always made the experiment with that, as being one of the pureſt clays. From this experiment it is certain, that a mixture of calcareous earth and clay is reducible to glaſs, in a degree of heat which is not ſufficient to vitrify either of theſe ſubſtances when pure. The clay, of which bricks and tiles are made, often contains a quantity of calcareous earth, and hence we may underſtand the reaſon why ordinary bricks and tiles are not able to reſiſt a great degree of heat: a tile covering a crucible is often quite vitrified, whilſt the crucible remains unaltered, the tile being a mixture of clay and calcareous earth, and the crucible being made of pure clay and ſand.

It is well known, that lime is lighter, bulk for bulk, than the chalk or ſtone from which it is made, ſome ſubſtance therefore or other is diſſipated during the calcination; the nature of this ſubſtance will be inquired into hereafter, at preſent our concern is to aſcertain its quantity. The ſubſtances which I converted into lime, were fine white marble called ſtatuary marble; Sienna marble; fine tranſparent cryſtallized ſpar, of that ſpecies which mineralogiſts have denominated hexagonal truncated; rhomboidal opaque refracting ſpar; rhomboidal tranſparent refracting ſpar; all theſe ſpars from Derbyſhire; a marble, of a blueiſh caſt, called dove-marble; purple-veined marble; Clitheroe limeſtone; Purbeck ſtone; chalk from Cherry Hinton, near Cambridge; Portland ſtone; Ketton ſtone from Rutlandſhire, denominated *hammites*, from its being compoſed of globules reſembling the roes of fiſhes. A common die is a little cube; if we ſuppoſe the length, breadth, and thickneſs of a die, to be reſpectively increaſed, till each of them becomes equal to an inch, foot, yard, or mile, then will the die become a cubic inch, a cubic foot, yard or mile. The weight of a cubic foot of water is found, by experiment, to be preciſely equal to 1000 avoirdupois ounces. Aſſuming then the weight of a cubic foot of water to be 1000 ounces, by a method, ſufficiently explained in books which treat of Hydroſtatics, I calculated what would be the weight of a cubic foot of each of the calcareous ſubſtances, which I converted into lime.

Weight of a cubic foot of

Water	1000	Avoirdupois ounces.
Purple-veined marble	2750	
Sienna marble	2729	
Statuary marble	2725	
Cryſtallized ſpar	2701	
Rhomboidal opaque ſpar	2700	
Dove-coloured marble	2694	
Rhomboid. tranſparent ſpar	2693	
Clitheroe limeſtone	2686	
Purbeck ſtone	2680	
Chalk, near Cambridge	2657	
Portland ſtone	2461	
Ketton ſtone	2456	
Medium of the 12 ſorts	2661	

It is unneceſſary to trouble the reader with a particular detail of each experiment, in which one or other of theſe ſubſtances was converted into lime; he will judge it ſufficient to be informed of the general method of proceeding, and to be made acquainted with the general reſult. The method conſiſted in ſelecting pure pieces of the ſeveral ſubſtances, and putting ſometimes two, ſometimes three of them into a crucible filled with ſand; care was taken that the pieces neither touched each other nor the ſide of the crucible, the crucible was then expoſed to a very ſtrong fire for four hours: none of the pieces weighed more than an ounce, ſo that the fire to which they were expoſed, it is apprehended, was quite ſufficient to convert them into perfect quicklimes; they were all dry, and were weighed very accurately before and after calcination, and in the weighing after calcination great care was taken to free them from every particle of ſand, and to weigh them before they were quite cold. The following table comprehends in one view the different reſults of the ſeveral experiments; the firſt column mentions the different ſorts of calcareous ſubſtances with which the experiments were made; the ſecond expreſſes in hundred weights, quarters, and pounds, the quantity of lime which a ton of each reſpective ſubſtance would yield by calcination; theſe numbers were derived, by the rule of proportion, from the ſmall weights uſed in the experiments;

ments; the third column exhibits the weight of the matter diſſipated from each calcareous ſubſtance during the calcination.

	C.	q.	lb.	C.	q.	lb.
Purple-veined marble	11	1	24	8	2	4
Purbeck ſtone - -	11	1	22	8	2	6
Clitheroe limeſtone -	11	1	17	8	2	11
Dove-coloured marble	11	1	13	8	2	15
Chalk - -	11	1	9	8	2	19
Sienna marble - -	11	1	7	8	2	21
Portland ſtone - -	11	1	4	8	2	24
Ketton ſtone -	11	1	2	8	2	26
Statuary marble -	11	1	1	8	2	27
Rhomboidal tranſp. ſpar	11	1	0	8	3	0
Rhomboidal opaque ſpar	11	0	12	8	3	16
Cryſtallized ſpar -	11	0	1	8	3	27
Med. of lime from a ton	11	1	$4\frac{2}{3}$			
Medium loſs of weight in a ton	8	2	$23\frac{1}{3}$			

Having formed this table from my own experiments, I was willing to ſee what correſpondence it bore with the experiments of others. Authors ſeldom expreſs themſelves on this ſubject with ſufficient preciſion; I have met, however, with ſome experiments, which ſeem to have been made with great accuracy, and I have from them calculated the following table upon the ſame principle as the preceding.

	C.	q.	lb.	C.	q.	lb.
* Courcelles limeſtone	11	2	2	8	1	26
‡ Calcareous earth ſeparated from Marle	11	1	23	8	2	5
‖ Calc. earth from Marle	11	1	20	8	2	8
† Chalk - -	11	1	9	8	2	19
§ Vienna limeſtone	11	1	0	8	3	0
* Courcelles limeſtone	11	0	24	8	3	4
¶ Swediſh limeſtone	11	0	18	8	3	10
¶ Gothland coral -	11	0	12	8	3	16
¶ Fiſh ſhells - -	11	0	6	8	3	22
* Courcelles limeſtone	11	0	1	8	3	27
¶ Chalk - -	11	0	0	9	0	0
§ Vienna limeſtone	10	2	14	9	1	14
Med. of lime from a ton	11	0	$20\frac{1}{12}$			
Medium loſs of weight in a ton -	8	3	$7\frac{11}{12}$			

From

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* Paris is principally ſupplied with lime from the Courcelles limeſtone,—the three pieces here mentioned were burned in an ordinary lime kiln for 30 hours. Mem. de l'Acad. 1747, p. 62.

From the bare inſpection of theſe tables it is obvious, that calcareous ſubſtances of different kinds and countries, do not differ much from each other in the quantity of lime which a ton, or any other definite weight of them would yield by calcination.

Though I had every reaſon to believe, that the experiments from which I calculated the tables were made with as much accuracy as the nature of the ſubject would admit; yet

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‡ Philoſ. Tranſ. 1773, p. 161.

|| Edin. Eſſ. Vol. III. p. 16.

† Dr. Black, to whom we are indebted or our knowledge of the conſtituent parts of alcareous earth. Edin. Eſſ. Vol. II.

§ Prof. Jacquin's Exam. Chem. Doc. Mey. Vienna, 1769.

¶ Wallerius, Swed. Mem. 1760.

yet I thought it would add greatly to their confirmation, if it could be ſhewn, that the quantity of lime really procured from the calcination of a ton of chalk or limeſtone, was not much different from the medium quantity aſcertained by the experiments. I went, therefore, to one of the kilns of this country, where they burn chalk into lime; and I firſt weighed four buſhels of chalk, which had been dried by the heat of a hot ſummer; the medium weight of the four buſhels was 86$\frac{1}{2}$ pounds: I then weighed four buſhels of the lime, warm from the kiln, the medium weight of which was 66$\frac{1}{4}$ pounds. Calculating, from theſe numbers, the weight of lime which a ton of chalk would yield, it will be found to amount to 15c. 1q. 7lb.

—This quantity of lime being ſo different from what I expected, I brought a piece of the chalk to my elaboratory. I then weighed it accurately, and found that a month after, though the weather was hot, it had neither gained nor loſt weight. then converted it into lime, and it gave after the rate of 11c. 1q. 19lb. of lime from a ton; or 3c. 3q. 16lb. leſs than what the ſame chalk when burned in the kiln would yield. I take this difference to be wholly attributable to the ſuperior degree of heat, with which the chalk in my experiment was burned. It is to be wiſhed that more coal was uſed in the urning of lime in this country; for a ton of chalk, when properly burned, ought to weigh only about 11 hundred weight, and it does ordinarily weigh about 15 hundred weight, it is obvious that the purchaſer has about 4 parts in 15 which are not lime but chalk; and to this cauſe are to be referred the hard lumps, which are found in the lime of this country, when it is ſlacked; theſe lumps being nothing but chalk which is not burned into lime for want of a ſufficient fire; the expence alſo of carrying 4 tons of chalk in every 15 tons of lime, is a circumſtance not to be neglected.

At Pennybridge, in Lancaſhire, they burn a hard limeſtone, a Wincheſter buſhel of this ſtone weighed 184lb. The ſame meaſure of the lime, when juſt burned, weighed 109 lb. Theſe numbers give 11 c. 3 q. 11 lb. of lime from a ton of the ſtone.

The lime burned at Clitheroe, in Lancaſhire, is reckoned to be as good as any in the kingdom, when applied either as a manure or as a cement: a buſhel up heaped of the beſt crude ſtone weighed 147 pounds, an equal meaſure of the ſame ſtone after calcination weighed 71 pounds; from this proportion it follows, that the Clitheroe limeſtone would give only 9 c. 2 q. 17 lb. of lime from a ton of the ſtone,—this concluſion might be rendered more certain if the medium weight of ſeveral buſhels was taken. It may, however, be expected that the Clitheroe limeſtone ſhould loſe more weight than the Cambridgeſhire chalk does, for in burning it they uſe at leaſt twice as much coal, or one meaſure of coal to two of limeſtone. This proportion of coal is a very great one. At Knareſborough, where they burn a kind of limeſtone formed by petrefaction, they uſe 1 buſhel of coal to 3 of lime.

From what has been ſaid, it muſt be concluded, that calcareous ſubſtances have a very great reſemblance to each other, as to the quantity of that matter which is diſperſed from equal weights of them during calcination; and here a curious queſtion preſents itſelf, which cannot be eaſily decided:—Is the difference in the quantity of lime, procurable from equal weights of different calcareous ſubſtances, to be accounted for, from the different proportion in which the earth remaining after calcination is united, in the crude ſubſtance, with the volatile part diſperſed during calcination; or does it proceed from ſome unavoidable inaccuracy in making the ſeveral experiments?—It muſt be acknowledged, that it is difficult to ſay whether the bodies are equally dry before they are ſubmitted to calcination, unleſs we knew the preciſe degree of force with which they imbibed and retained the humidity of the atmoſphere; it is difficult alſo to give them exactly the ſame degree of heat during their calcination, and to weigh them whilſt they are equally warm; the greateſt attention, moreover, cannot always prevent a loſs of ſome part of the ſubſtance to be weighed, nor a gain in weight from the accretion of ſome heterogeneous matter; yet, notwithſtanding theſe, and, perhaps, other ſources of error in making the experiments, I am rather diſpoſed to think, that the proportion in which the fixed earth is united to the volatile ſubſtance, is not the ſame in all calcareous bodies.

A piece of dry chalk, and the three Derbyſhire ſpars were put into the ſame crucible, and expoſed for four hours to the ſame fire; it appeared, from the experiment, that a ton of the chalk would have given 36 pounds more of lime than a ton of the ſpar which gave the leaſt quantity of lime, and that the ſpar which gave the moſt lime from a ton, exceeds that which gave the leaſt by 27 pounds.

I made another experiment with the ſame view. Six different calcareous ſubſtances, which had been kept in the ſame room for a month, or more, were put into the ſame crucible, and expoſed to a very ſtrong fire for 24 hours. I weighed them all whilſt they were warm, and the quantities of lime which a ton of each would have given, with that degree of heat, are expreſſed in the following table:

	C.	q.	lb.
Chalk - -	11	1	6
Clitheroe limeſtone	11	0	22
Sienna marble -	11	0	8
Statuary marble	10	3	21
Rhom. ſpar -	10	3	2
Cryſtallized ſpar	10	1	26

From this experiment it may be inferred, that the difference between the quantities of lime earth, procurable, under ſimilar circumſtances, from a ton of chalk, and a ton of hard tranſparent cryſtallized ſpar, is 92 pounds, or near $\frac{1}{24}$ of the whole weight.

Whether this difference be greater than what might ariſe from different ſpecimens of the ſame *kind* of calcareous ſubſtance, is more than I dare poſitively affirm. One ſpecimen of the Courcelles limeſtone, we have ſeen, gave after the rate of 11 c. 2 q. 2 lb. from a ton of the crude ſtone; another calcined in the ſame kiln gave only 11 c. 0 q. 1 lb. from a ton, the difference is 57 pounds, or about $\frac{1}{40}$ of the whole weight. Different pieces broken even from the ſame lump of chalk, limeſtone, marble, and almoſt every other calcareous ſubſtance, ſeldom conſiſt of preciſely the ſame materials; this is obviouſly ſeen in buildings and pavements, made either with marble, Ketton, or Portland ſtone; there may generally be obſerved hard knobs, which do not wear away ſo faſt as the other parts of the ſtone; and thoſe who are accuſtomed to ſaw any of theſe ſubſtances are fully ſenſible of this diverſity of ſtructure in the ſame block of ſtone, from the greater difficulty of cutting ſome parts of it than others. I broke the ſame ſmall lump of chalk into four different pieces, and calcined them for different lengths of time, with a view of ſeeing whether there was not ſome regular proportion with reſpect to the time according to which the quantity of lime procurable from chalk would vary. I broke alſo the ſame lump of Clitheroe limeſtone into four ſmall pieces, and calcined them for different lengths of time with the ſame intention; the reſult of the experiments is expreſſed in the annexed table.

	Lime from a ton of		
Chalk calcined 4 hours.	11	1	21
7 hours	11	2	18
10 hours	11	2	14
24 hours	11	1	6
Clitheroe limeſtone calc. 4 hours	11	1	17
7 hours	11	0	1
10 hours	11	1	9
24 hours	11	0	22

The reader will perceive that theſe experiments did not anſwer my intention in making them, ſince there does not appear to be any regular decreaſe of lime with the increaſe of the time of calcination; but they probably point out to us a difference in the compoſition of the different ſpecimens; and, indeed, in the chalk which was burned for 7, as well as in that which was burned for 10 hours, I could diſcern, after calcination, ſome ſmall ſpecks of iron which were wholly inviſible before calcination.

Mr. Whitehurſt * has favoured the world with ſome very intereſting obſervations, concerning the *ſtrata* in Derbyſhire; the following is his account of their reſpective thickneſſes, and of the order in which they ſucceed one another; No. 1. denoting the uppermoſt *ſtratum*.

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* See his Appendix to the Inquiry into the State of the Earth.

1. Grit	120	yards
2. Shale	120	
3. Limeſtone	50	
4. Toadſtone	16	
5. Limeſtone	50	
6. Toadſtone	46	
7. Limeſtone	60	
8. Toadſtone	22	
9. Limeſtone not cut through.		

Theſe ſeveral ſtrata of limeſtone, are each of them divided into layers, which have very different appearances. Thus No. 3. which is 50 yards in thickneſs, may be conſidered as conſiſting of 5 diſtinct layers. The 1ſt layer, which is contiguous to the ſhale, is the black Derbyſhire marble; the 2d is called by the miners, ſnail horn marble, from its abounding with ſcrew-ſtones, and fiſh ſhells; the 3d is a compact grey limeſtone, with ſeveral large cryſtalline ſpecks in n it, but with few ſhells; the 4th as rather leſs ſpecks, and is whiter han the 3d; the 5th layer, which ies immediately over the toadſtone, s likewiſe a grey limeſtone. I examined the relative weights of the lack marble, and of the ſecond, third nd fourth layers of the ſtratum, No. . with great attention, and alſo the uantities of lime which a ton of each ould yield; the ſeveral ſpecimens ere calcined in the ſame crucible, or the ſame length of time.

eight of a cubic foot of the crude ſtone.		Weight of lime from a ton.		
	Ounces.	C.	q.	lb.
Layer 1. -	2690	11	2	7
2. -	2685	11	1	7
3. -	2640	11	0	15
4. -	2650	11	1	4
Medium	2666	11	1	8$\frac{1}{4}$

It appears from theſe experiments, that theſe 4 layers of the ſame ſtratum of limeſtone, do not lie in the earth according to their weights, the uppermoſt layer being the heavieſt, and the third layer being lighter than that either immediately above or below it. The quantity of lime, which a ton of each would yield, ſeems to have ſome dependence on their weights; the heavieſt layer yielding the moſt, and the lighteſt layer yielding the leaſt lime from the ſame weight of ſtone *.

I have

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* This obſervation is not true, I think, concerning calcareous ſubſtances in general, for a piece of Derbyſhire watricle, which was 30 ounces in a cubic foot heavier than the Derbyſhire black marble, being calcined in the ſame crucible with the marble, gave only 11 c. 0 q. 11 lb. of lime, which is 52 pounds in a ton leſs than the marble gave.

I have now given an account of 2 experiments made by other au- hors, and of 32 made by myſelf, on alcareous ſubſtances of different inds and countries, the medium uantity of lime procurable from a on of theſe ſubſtances, will be found o amount to 11 c. 0 q. 25lb. and onſequently the medium loſs of reight will be 8 c. 3 q. 3lb.

When compact maſſes of any ſort f lime are expoſed to the air, even a room ſheltered from rain or dew, ey ſoon begin to crack, and in no reat length of time they fall into a owder, and receive a conſiderable creaſe of weight. The time requi- te for effecting this change, depends uch upon the quantity of the ſur- ce of the lime, which is expoſed to e air, partly, I believe, but I am not certain, upon the degree of humidity reigning in the atmoſphere, and partly upon the nature of the body. A piece of ſtatuary marble was converted into lime on the 10th of Feb. 1779; before its calcination it weighed 540 grains, after calcination and whilſt it was yet warm from the fire, it weighed 304 grains; this laſt quantity was laid upon a piece of clean paper, put into a drawer of my ſtudy table, and weighed at different intervals, till it had acquired its utmoſt increaſe of weight, which it attained on the 4th of March, it then weighing 515 grains. I weighed this lime for two months afterwards, at different times, but it ſtill weighed the ſame. Another quantity of lime from ſtatuary marble, was examined in the ſame way, and it acquired its greateſt increaſe of weight in twenty-two days. It did not ſeem to me, that any other kind of calcareous ſubſtance, when reduced to lime and expoſed to the air, acquired its greateſt increaſe of weight ſo ſpeedily as ſtatuary marble; I make this obſervation with diffidence, for I have neither tried all the ſorts of calcareous ſubſtances, nor any of them under preciſely the ſame circumſtances. There is far greater trouble in making experiments, than thoſe, who have not been accuſtomed to the buſineſs, can readily conceive; many niceties are to be attended to, the leaſt of which being omitted, the concluſion becomes doubtful; the mind, moreover, having once acquired the ſtriking outlines of knowledge, has not always

patience

patience to attempt the filling up the minuter part of the deſign, eſpecially if its attention can be but accidentally employed upon the ſubject.

Whoever undertakes to aſcertain the time in which lime, prepared from different ſubſtances, acquires its greateſt increaſe of weight by expoſure to the air, would do well to take equal quantities of the limes, whilſt warm from the fire in which they were made, to reduce them immediately to powders equally fine, to expoſe the powders to the air, in a room free from duſt, and to weigh them at ſtated intervals. Or the experiment may be varied by taking, inſtead of powders, equal in weight and fineneſs, ſolid pieces equal in bulk and ſurface, ſuppoſe a cubic inch of each ſort, and I queſtion not but ſome intereſting concluſions, relative to the attractive powers of the different ſubſtances, might be derived from experiments of this ſort, carefully and repeatedly made.

The powders into which the limes of different ſubſtances are reſolved by expoſure to the air, have not all the ſame appearance; thoſe of ſtatuary marble, dove marble, Portland ſtone, Ketton ſtone, and, probably, ſeveral other ſorts, are compoſed of little globules viſible to the naked eye; whilſt thoſe of chalk, Sienna marble, and ſpar, have no appearance of globules, nor do they feel ſo harſh and gritty to the touch, as the others do. The whiteſt of all the limes, which I have ever ſeen, is that which may be obtained from the cryſtallized ſpar before mentioned; ſtatuary marble, and Portland ſtone, give very white limes, but they are many degrees leſs white, than that from cryſtallized ſpar. The limes from Sienna marble, chalk, rhomboidal ſpar, and Ketton ſtone, have a yellowiſh caſt.

Several experiments were made in order to aſcertain the utmoſt increaſe of weight, which different ſorts of lime can acquire by expoſure to the air. The limes were all looſely folded up in clean paper, laid in the cloſet of a room where there was a conſtant fire, and weighed at different intervals, till they had acquired their greateſt increaſe of weight, which ſome of them did not do in leſs than three quarters of a year. The general reſult of the experiments is expreſſed in the following table; where the firſt column contains the names of the ſubſtances, from which the limes were prepared, the ſecond expreſſes the height to which a ton of each ſort of lime would increaſe by expoſure to the air, and the third ſhews the quantity of the increaſe in a ton.

	C.	q.	lb.	C.	q.	lb.
Sienna marble - -	35	0	12	15	0	12
Dove marble - -	35	0	9	15	0	9
Portland ſtone - -	34	3	21	14	3	21
Statuary marble -	34	2	0	14	2	0
Rhomboid. tranſ. ſpar	34	0	16	14	0	16
Purbeck ſtone - -	34	0	8	14	0	3
Cryſtallized ſpar -	33	1	25	13	1	25
Rhomb. opaque ſpar	32	1	21	12	1	21
Clitheroe limeſtone	32	1	15	12	1	15
Ketton ſtone - -	31	3	13	11	3	13
Chalk - - -	30	2	9	10	2	9

Hence it is evident, that the greateſt increaſe of weight which a ton of lime, of the ſeveral ſorts here mentioned would acquire by expoſure to the air, amounts to rather more than three quarters of a ton, and the leaſt amounts to above half a ton. This obſervation is worthy of notice in a practical view; lime is ſometimes ſold by weight, and it is obvious, that the ſeller will be a very conſiderable gainer, if, after having drawn his kiln, he lets his lime remain expoſed to the air for a few days before it is ſold. I have frequently obſerved pieces of new-burned lime, daily increaſing at the rate of one hundred weight per ton, for the firſt five or ſix days. The farmer too, in liming his land, ſhould contrive to carry out his lime as ſoon as poſſible after it is burned, he may otherwiſe for every ton, have the trouble of carrying a ton and a half, or more.

Lime which has once acquired its greateſt increaſe of weight, does not loſe any part of what it has acquired by ſubſequent expoſure to the air, even during the heat of ſummer. Several ſorts of lime, which I apprehended had gained their greateſt increaſe of weight in April and May 1779, were weighed at different times during the ſummer of the ſame year which was frequently very hot, but I did not obſerve that any one of them was diminiſhed in weight by the action of the heat: let the matter, therefore, which is attracted by the lime from the air be what it may, it is a permanent ſubſtance, and not ſubject to evaporation during ſummer. Hence it cannot be doubted, that the ſoil upon which lime is ſpread acquires a great increaſe of matter; every ton of lime attracts above half a ton of ſome ſort of matter or other from the air, and adds it to the earth; now this matter would not have become united with the earth without the intervention of lime: but whether any part of the advantage of liming, where the ſoil is bad or ſcanty, may be attributed to this increaſe of the quantity of matter which is derived from the air to the earth, is a queſtion which I have not ſkill enough in agriculture to decide.

I have been told by a gentleman of fortune in Derbyſhire, that he has frequently, with great ſucceſs, ſpread 1000 Wincheſter buſhels of lime upon an acre of ground: let us ſuppoſe a Wincheſter buſhel of Derbyſhire lime to weigh 100 pounds, then will 1000 buſhels weigh 100000 pounds, or above 44 tons; and ſuppoſing one ton of ſuch lime to gain only 14 hundred weight by expoſure to the air, then will the whole acre receive an increaſe of ſoil, from the lime which is ſpread upon it, equal to above 30 tons in weight beyond the weight of the lime.

Having eſtimated the loſs of weight ſuſtained by different calcareous ſubſtances during their calcination, and alſo the gain of weight which the limes procured from theſe ſubſtances acquire by expoſure to the air, it is natural to inquire whether the gain is equal to the loſs. Upon the 6th of March I converted into lime 204 grains of dove marble; the lime weighed whilſt hot, 116 grains; upon the 5th of the following November the lime weighed 203½ grains. This lime was frequently weighed from March to November, and if it ſhould be ſuppoſed that half a grain was inſenſibly loſt by frequent weighing, then we muſt conclude that this kind of lime gains by expoſure to the air, juſt as much as the marble it is prepared from loſes by calcination. Upon a repetition of this experiment with another ſpecimen of the dove-marble lime, I found the reſult to be, as accurately as can be expected, the ſame. If we make no allowance for any loſs of weight by repeated weighings, then the increaſe of weight by expoſure to the air muſt be eſteemed leſs than the loſs of weight by calcination, by about one four hundredth part of the weight of the marble.

Some dry chalk, weighing 120 grains, was reduced by calcination to 68 grains of lime; this quantity of lime became, by expoſure to the air, when it had acquired its utmoſt increaſe of weight, 104 grains; that is, it wanted 16 parts in 120, or between one ſeventh and one eighth of the original weight of the chalk.

The dove marble and the chalk are the two extremes of all the ſubſtances I have tried; no kind of lime regained ſo much weight as that from dove marble, and none ſo little as that from chalk. Philoſophy may be well employed in accounting for this diverſity; I am only intereſted at preſent in eſtabliſhing the fact, which ſeems not to have been ſufficiently underſtood by chemiſts of deſervedly great eminence. M. Macquer ſays "quicklimé combines with the moiſture of the air which it ſeems capable of attracting, by the moiſture thus attracted, the weight of the quicklime is doubled*." M. Baumè weighed ſome lime very accurately, and though he kept it in a cellar a cellar for ſeveral years, yet it never regained the weight of the calcareous earth, from which it had been prepared; there was always a deficiency of more than two ounces in a pound*.

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* Chem. Dict. by M. Macquer, artic. Quicklime in the Engliſh tranſlation:—The original of this work is excellent in its kind, and it is greatly improved by the very judicious notes of the tranſlator. In juſtice to M. Macquer, it ought to be obſerved, that in the ſecond Engliſh Edition of his Dictionary, the paſſage quoted in the text is greatly altered, there is no mention made of the *moiſture* of the air, and the quicklime is only ſaid to recover a *great part* of its original weight, by expoſure to the air.

I do not queſtion the veracity of either of theſe gentlemen, or their accuracy in making their experiments; but the properties of the particular limes which they examined ſhould not have been given as characteriſtic properties of limes in general. No lime that I ever tried was doubled in weight by attracting matter (be it moiſture or other matter) from the air, yet if a limeſtone could be found, which is not impoſſible, that loſt half its weight by calcination, a ſpecimen of lime from ſuch a limeſtone

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* Chymie par M. Baumè, Vol. I. p. 177.

ſtone might, perhaps, be doubled in its weight. On the other hand, I only met with one ſort of lime, that from chalk, which in its increaſe ſeemed to fall ſo far ſhort of the original weight of the ſubſtance from which it was made, as to want more than two ounces in a pound, or between one ſeventh and one eighth of the whole weight. My experiment in this particular inſtance, ſufficiently coincides with the too general obſervation of M. Baumè.

The chalk or clunch of this country is not a pure calcareous earth, it contains a portion of ſand; and hence, when a definite weight of this chalk lime is expoſed to the air, it cannot acquire as much increaſe of weight, as the ſame quantity of purer lime would do: for the ſand, which is mixed with the lime, is of a very different nature from the lime, and does not attract any thing from the air. This obſervation may be extended to different ſorts of lime-ſtone, and it may ſuggeſt one reaſon for the diverſity, in the ſtrength of different ſorts of lime, when uſed either in building or in agriculture.

The table in which the increaſe of weight acquired by different ſorts of lime, in conſequence of their expoſure to the air, is expreſſed, was made with great accuracy; yet is it not to be conſidered as univerſally true, even of the ſubſtances there mentioned: for there is great reaſon to believe, that the quantity of matter, which the ſame ſort of lime can attract from the air, depends very much upon the degree of heat with which the lime has been burned; there is a certain definite degree of heat requiſite to make the lime attract the greateſt poſſible quantity from the air. When the heat is not ſufficient to convert the whole of the ſubſtance into lime, then that part which is not changed into lime, will not contribute any thing to the increaſe of the weight of the lime when expoſed to the air, inaſmuch as it poſſeſſes no quality of attracting any thing from the air. And, I believe, the obſervation is equally true, when the heat has been *more* than ſufficient to convert the whole of the ſubſtance into lime. It has been before mentioned, that ſix different calcareous ſubſtances were changed into lime, by being expoſed to a very fierce fire for 24 hours: this experiment was made in November, 1779; I have frequently weighed the limes ſince that time; they have all of them acquired their greateſt increaſe of weight; for on weighing them in January, 1781, I found that they had not gained any increaſe of weight in an interval of three months. The following table expreſſes, in the ſecond column, the increaſe of weight which a ton of theſe ſubſtances would have gained, and the third column expreſſes the increaſe which a ton of the ſame ſubſtances would have gained, had they been only expoſed to the ſame fire for 4 hours.

	C.	q.	lb.	C.	q.	lb.
Sienna marble -	8	1	15	15	0	12
Rhomb. ſpar -	8	1	5	14	0	16
Statuary marble -	8	0	0	14	2	0
Chalk - -	7	1	13	10	2	9
Clitheroe limeſtone -	6	2	9	12	1	15
Cryſtallized ſpar -	6	0	0	13	1	25

Theſe experiments clearly prove the great utility of burning lime with a definite heat, for the excellency of lime, to whatever purpoſe it be applied, depends, probably, upon its diſpoſition to attract that matter from the air, which it has loſt by calcination; and we ſee that none of the limes, which were burned for 24 hours, attracted ſo much from the air, as thoſe did which had been more moderately burned; the difference is very conſiderable in all the inſtances. Would not a greater degree of heat have ſtill further deprived lime of its property of attracting matter from the air, and thus have made it approach nearer to the nature of glaſs?

We have ſeen how greatly calcareous ſubſtances are diminiſhed in weight by being calcined,—do they receive any alteration of bulk during that operation? Having propoſed this queſtion to various lime-burners, in different parts of the kingdom, I found ſome were of opinion that the chalk or ſtone ſwelled a little, others that it ſhrunk a little during calcination, and a third ſet thought that it underwent no ſort of alteration: the moſt obvious inference from theſe different accounts ſeems to be, that calcareous ſubſtances do in this particular differ from each other.

I took a piece of blackiſh marble four inches in length, half an inch broad, and one quarter of an inch thick, and having placed it in a proper veſſel, and covered it with ſand, it was expoſed to a very fierce fire for three hours; the marble thereby became perfectly white and friable as calcined bones, but I could not obſerve that it had undergone the leaſt change in any of its dimenſions.

I cut a piece of chalk till it was an inch in length, half an inch in breadth, and as much in thickneſs; after it was calcined, its dimenſions were not ſenſibly altered.

A piece of rhomboidal ſpar from Derbyſhire, and a piece of white marble, were ſeverally meaſured before and after calcination, but no difference could be obſerved in the dimenſions of either of them.

I thought the length of a piece of Sienna marble, in one experiment, was diminiſhed about one fiftieth part by calcination, but I was probably miſtaken; for in another piece, above two inches in length, I took, with a pair of compaſſes, the diſtance between two points upon its ſurface before calcination, and found that diſtance preciſely the ſame after calcination.

A piece of Clitheroe limeſtone, which had been calcined for four hours, appeared to be diminiſhed in length one part in twelve; another piece which had been calcined ten hours, was unaltered in length, and thickneſs; and ſo was a piece which had been calcined 24 hours.

I took a piece of chalk, which had been dried by the ſun and air, and having ſhaped it into a ſquare, each ſide of which was four inches, the thickneſs being two inches, I put it into a lime-kiln; it was kept there as long as chalk which is burned into lime uſually is; when it came out of the kiln, I compared it with the ſtandards of its ſeveral dimenſions, which had been accurately taken before calcination, and could not diſcover that it had ſuffered the leaſt change either in length, breadth, or thickneſs. This experiment was repeated with wet chalk, but neither was this altered in its dimenſions by calcination. Laſtly, I took a flat piece of chalk, about one fourth of an inch in thickneſs, and calcined it for 24 hours; the length of this chalk was three inches, and it was diminiſhed by calcination one tenth of its length.

The reader is poſſeſſed of all I know from my own experiments upon the ſubject; but I will not conceal from him the opinion of M. Baumè, who eſteems the diminution of the bulk of calcareous ſubſtances during calcination, as one of their moſt diſtinguiſhing properties. He does not, indeed, mention any experiments which he had expreſſly made, with a view of deciding the queſtion, but I cannot ſuppoſe, that ſo able a chemiſt would ſpeak from conjecture, when he affirms, that calcareous ſubſtances are ſometimes diminiſhed half their bulk during calcination *; and therefore I ſhould apprehend, that the calcareous ſubſtances, which he uſed, were different from any of theſe on which I made my experiments. Be this as it may, his concluſion is, if at all, certainly not generally to be admitted.

The experiments, by which the nature of any particular ſort of earth is chemically inveſtigated, are principally of two kinds; one conſiſts in obſerving its ſolubility or inſolubility in different menſtruums, the other in noticing

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* Le ſecond phénomene que la terre calcaire préſente après ſa calcination, eſt ſa diminution de volume——la retraite de ces parties, pendant leur calcination, eſt quelque fois ſi grande, qu'elle va juſqu' à la *moitié* de leur volume. Chym. par M. Baumè, Vol. I. p. 182.

noticing the changes produced in it by the action of various degrees of heat. We may form a general idea of the operation of fire upon calcareous ſubſtances, from the experiments which have been before related; I did not ſpare either time or labour, nor was I ſenſible of any want of care in making them; yet I cannot help apprehending, that a numerous repetition of ſimilar experiments might lead to ſome general concluſions, which have eſcaped my obſervation. The following experiments can hardly fail of being acceptable to the reader, as they tend greatly to corroborate what has already been inferred concerning the nature of calcareous earths. We have concluded, that twenty hundred weight, or a ton of any calcareous ſubſtance, conſiſts of between eight and nine hundred weight of ſome matter or other, which is diſſipated during calcination, and of between eleven and twelve hundred weight of earth, which cannot be diſſipated in that degree of heat: now if it can be ſhewn, by a way wholly different from that of calcination, that a ton of any calcareous body does really conſiſt of about twelve hundred weight of earth, and of about eight hundred weight of a volatile ſubſtance, we cannot entertain any further doubt concerning the proportion of the conſtituent parts of calcareous ſubſtances.

If the reader has not been accuſtomed to the making of chemical experiments, and yet ſhould be deſirous of aſcertaining, from his own obſervation, the truth of ſuch as I am going to mention, he may do it at a very eaſy rate; he need only procure from his apothecary, a few ounces of the acid of ſea ſalt, and a Florence flaſk, and he will be furniſhed with the main part of the apparatus.

I took a Florence flaſk, and poured into it a ſmall portion of the acid of ſea ſalt diluted with water; the flaſk and its contents were then weighed in a pair of ſcales, which turned with a quarter of a grain; the weight was noted down. I then dropped into the acid, by a very little at a time, 20 grains of a calcareous ſubſtance, and ſtopping gently the top of the flaſk with my finger, till the whole was diſſolved, the flaſk was weighed again. Now it is obvious, that if nothing had eſcaped during the ſolution, the weight of the flaſk and its contents, after the ſolution of the calcareous ſubſtance, would have exceeded its weight before ſolution by 20 grains. There was, however, a very conſiderable loſs of weight, in all the ſubſtances which I tried.. If inſtead of 20 grains, a ton, or 20 hundred weight of each ſubſtance, had been diſſolved, and no notice had been taken of any weight leſs than $\frac{1}{4}$ of an hundred weight, the weight of the earth, or other matter remaining in the bottle after ſolution, would have been expreſſed by the ſecond column of the following table, and the weight of the matter loſt during ſolution by the third.

Namure

	C.	q.	lb.	C.	q.	lb.
amure black marble	13	3	0	6	1	0
litheroe limeſtone	13	1	0	6	3	0
halk	12	3	0	7	1	0
etton ſtone	12	3	0	7	1	0
ortland ſtone	12	1	0	7	3	0
urbeck ſtone	12	1	0	7	3	0
other of Pearl	12	2	0	7	2	0
ove marble	12	0	0	8	0	0
lue-veined marble	12	0	0	8	0	0
urple-veined marble	12	0	0	8	0	0
tatuary marble	12	0	0	8	0	0
homboidal ſpar	12	0	0	8	0	0
ryſtallized ſpar *	12	0	0	8	0	0

The

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* Six out of the thirteen ſubſtances here ex-
ined, loſt 8 parts in 20 during their ſolution,
ich is the very proportion aſſigned by Dr.
ack, in his moſt ingenious Eſſay (Edin. Eſſ.
l. II. page 104), for the loſs ſuſtained by
alk; it will appear, however, from what fol-
ws, that pure calcareous ſubſtances probably
ſe 9 parts in 20 of their weight, by being diſ-
ved in an acid.

The Namure black marble and the Clitheroe limeſtone, threw up an oily ſcum during ſolution, and left a large portion of black ſediment, which probably was iron. The chalk had a whitiſh ſandy ſediment, the Ketton ſtone a yellowiſh one, Portland and Purbeck ſtone a white one, which looked like particles of ſand, and was gritty between the teeth; Mother of Pearl left ſeveral ſoft inſoluble flakes floating on the top of the acid, the other bodies either gave no ſediments, or very minute ones.

When I firſt began making theſe experiments, I was accuſtomed to weigh the flaſk as ſoon as the ſolution was ended, and this led me into an error in eſtimating the quantity loſt during ſolution; for, upon further trial, I always found, that the weight of the flaſk and its contents, taken, as accurately as poſſible, as ſoon as the ſolution was finiſhed, was diminiſhed very ſenſibly in the ſpace of three or four hours. I was at firſt inclined to attribute this diminution to the evaporation of a part of the fluid; but the true cauſe is this, the weight of the air remaining in the flaſk after ſolution, is greater than the weight of an equal bulk of atmoſpherical air. This is eaſily proved, either by ſucking through a pipe the air contained in the flaſk, or blowing it out with a pair of bellows, ſo that the flaſk may be filled with common air; for the flaſk will then be found to weigh leſs than it did before. Sucking out the air is a troubleſome operation, for the air has a diſagreeable taſte in the mouth, and if ſuffered to paſs into the lungs, might be noxious. This diſagreeable taſte is not to be attributed to any ſubtle particles of the acid, which may be ſuppoſed to float in the inſide of the flaſk, but to the air contained in it; for after the air has been thoroughly exhauſted, ſo that the flaſk becomes full of only common air, no further taſte is perceived.

I took a Florence flaſk, which would hold 26 ounces troy of water; into this flaſk I poured a two-ounce meaſure of the acid of ſea ſalt, and diſſolved ſlowly in the acid a quantity of the pureſt calcareous ſpar, the bottle was weighed as ſoon as the ſolution was ended: I then permitted it to ſtand quiet for two hours, and at the expiration of that time, I blew into it with a pair of bellows, till the air became taſteleſs; the bottle was then weighed, and it had loſt 7 grains. The weight of common air filling a flaſk, holding 24 ounces of water, is about 14 grains; the weight of the ſame quantity of the kind of air which filled the flaſk, immediately after the ſolution of the calcareous ſubſtance, was 7 grains more, or 21 grains; hence the weight of common air, is to that of the air which filled the flaſk after ſolution, as 14 to 21, or as 1 to 1½. I do not offer this concluſion to the reader as an accurate one, but it is probably not far from the truth.

The circumſtance of the weight of the flaſk being greater, immediately after the ſolution was finiſhed, than it would have been if filled only with common air, was not neglected in making the experiments mentioned in the table; it was not, however, attended to with ſuch a ſcrupulous exactneſs, as it ought to have been: I made therefore the four following experiments with the greateſt care. The ſeveral ſolutions were ſlowly made, to prevent any loſs of the acid, and the flaſk was not weighed till 20 hours after the ſolution was finiſhed; in which time the air in it had become taſteleſs; nor did the flaſk and its contents ſuffer any further diminution of weight, by a longer expoſure to the air.

120 gr. of white marble loſt by ſolut.	53½ gr.
120 gr. of cryſtal. ſpar loſt by ſolut.	54 —
120 gr. of rhomb. ſpar loſt by ſolut.	54 —
120 gr. of Matlock petrefaction —	54 —

The loſs of weight ſuſtained by the marble during ſolution is ſo nearly the ſame with that of the other three bodies, that its difference may probably be referred to ſome unavoidable inaccuracy in making the experiment; if this be admitted, we may reaſonably conclude that equal weights of pure calcareous ſubſtances, contain equal quantities of a volatile matter, which is diſperſed during ſolution, and that this volatile matter amounts to 54 parts in 120, or nine hundred weight in a ton, the earth amounting to 11 c.

Since calcareous ſubſtances loſe nearly equal portions of their weight, whether they are calcined in the fire, or diſſolved in acids, it may be preſumed that the matter which is loſt is of the ſame nature in both caſes; this matter is a ſpecies of air. The reader may eaſily convince himſelf of this by faſtening a flaccid bladder over the mouth of a Florence flaſk, or other veſſel, in which any calcareous ſubſtance is in the act of ſolution, for the bladder will preſently be blown up, by the matter which iſſues through the neck of the flaſk; and if the neck of the inflated bladder be tied, and the bladder be in that ſtate removed from the flaſk, it will not become flaccid again, but remain blown up, which is a ſufficient proof that it is filled with a ſpecies of air. The air thus ſeparated from calcareous ſubſtances, is very different from common air, in many particulars. We have before ſeen, that the weight of a given bulk of this air, is as much greater than the weight of the ſame bulk of common air, as 21 is greater than 14, or as 3 than 2.—The air, it has alſo been remarked, has a ſingular taſte, and common air has no taſte.—This air extinguiſhes flame and animal life, and common air ſupports both.—This air by being ſhaken in contact with water, or otherwiſe mixed with it, impregnates the water with an acidulous taſte, but common air produces no ſuch effect on water.—This air is as much a conſtituent part of the atmoſphere as common air is, for a ton of lime will attract half a ton of this air from the atmoſphere. The particles of matter conſtituting light, are ſuppoſed to be of different magnitudes: are the particles of matter which conſtitute atmoſpherical air of different magnitudes?—Does the ſmell of a room newly plaſtered, proceed from particles of matter emitted by the plaſter, or from the air of the room having undergone a decompoſition, in conſequence of the lime having abſorbed one of its component parts?

When any calcareous earth has been diſſolved in an acid, it has loſt its air, and is much diminiſhed in weight; it has undergone as great a decompoſition, as if it had been calcined. Calcined calcareous earth or lime, may be reſtored to the ſtate of crude calcareous earth or limeſtone, by expoſure to the air; in conſequence of ſuch an expoſure, it, in a greater or leſs ſpace of time, attracts from the air a part at leaſt of the principle which it had loſt during calcination; but when a calcareous earth has been diſſolved in an acid, and is thereby deprived of its air, it may be wholly reſtored to the ſtate of a crude limeſtone, by an almoſt inſtantaneous proceſs. Suppoſe 20 pennyweights of marble to have been diſſolved in a proper quantity of the acid of ſea ſalt, then will theſe 20 pennyweights have loſt nine parts of their weight; the earth remaining in the acid will only weigh 11 pennyweights. Salt of tartar, pot aſh, and other fixed alkalies contain, in their crude ſtate, a great portion of *fixed air*, of the ſame nature with that which conſtitutes nine parts in 20 of the ſubſtance of marble, and other calcareous bodies. The acid of ſea ſalt, and all other acids, have a ſtronger tendency to unite themſelves with the earthy part (if the expreſſion may be admitted) of fixed alkalies, than to continue united with the earthy part of marble, or any other calcareous body. If then into the acid, in which 20 pennyweights of marble have been diſſolved, and which really conſiſts of the acid united with 11 pennyweights of earth, we pour a ſolution of ſalt of tartar, or pot-aſh, the acid will quit the earth of the marble, and unite itſelf with one of the conſtituent parts of the alkali; the earth of the marble is by this means ſet at liberty, and one of the conſtituent parts of the alkali is alſo ſet at liberty, namely, its fixed air; theſe two will unite together, and by their union, they will form an earth of the ſame nature as crude limeſtone, the weight of which is found to be equal to the original weight of the marble, or 20 pennyweights.

From the various experiments which have been here faithfully related without any previous attachment to ſyſtem, it ſeems as if we might conclude, that pure calcareous ſubſtances are compoſed of 9 parts in 20 of a volatile ſubſtance, which is diſperſed, not only during the calcination, but during the ſolution of the ſubſtances in an acid, and of 11 parts in 20 of an earth which is generally known under the name of lime. Several authors of diſtinguiſhed reputation in Chemiſtry have thought, that *water* was an eſſential conſtituent part of calcareous ſubſtances, and from particular inſtances they have endeavoured to aſcertain its quantity in general.—The experiments which I have made do not tend to eſtabliſh this opinion.

—Into a new glaſs retort I put two ounces of cryſtallized ſpar, and having fitted a clean receiver of flint glaſs to it, I expoſed it at firſt to a gentle fire, which was increaſed, by degrees, till it became ſtrong enough to melt the retort; during the whole of the operation I did not obſerve that any water was ſeparated from the ſpar, the tranſparency of the receiver was not ſo much as tarniſhed by a vapour. From this experiment I would conclude, that this ſpecies of calcareous earth does not contain any water which is ſeparable from it in a degree of heat ſufficient to melt glaſs. I took an earthen retort, and put into it ſix ounces of cryſtallized ſpar, and expoſed the retort to a very ſtrong fire till the ſpar had loſt one third of its weight, there was not a drop of water found in the receiver. The retort, when taken out of the fire, appeared to be cracked, ſo that this experiment is not quite ſo convincing as the preceding, though, perhaps, ſufficiently ſo to make us doubt concerning the exiſtence of water as a component part of this kind of ſpar. —I took 720 grains of Derbyſhire watricle, which is the ſubſtance of which the ornamental columns ſeen on chimney pieces are generally formed, and diſtilled them with a violent fire in an earthen retort, till they were reduced to 400 grains (that is, till they gave after the rate of 11 c. 0q. 12 lb. of lime from a ton of the watricle), but I did not find any ſenſible portion of water in the receiver; it was indeed clouded with a vapour, but this vapour I think aroſe from the dampneſs of the retort, or of the ſubſtance uſed in faſtening the receiver to the retort, or from ſomething or other unconnected with the watricle.—

ESSAY VII.

OF CLAY, MARLE, AND GYPSEOUS ALABASTER, OR PLASTER-STONE.

ARgillaceous earth, or clay, may be diſtinguiſhed from every other ſort of earth, by its tenacity when wetted with water, and by its growing hard when expoſed to the fire. We may levigate ſand, chalk, or plaſter-ſtone into very fine powder, but theſe powders when kneaded with water, will not form a tenacious ductile maſs, capable of being moulded and turned at the pottery wheel, or of being hardened in the fire like clay. Even clay itſelf loſes its tenacity by being baked in a gentle fire: a potter or a pipe-maker will in vain attempt to ſubſtitute the powder of potſherds, or of broken pipes, in the place of clay; theſe powders may be ſerviceable on ſome occaſions, when mixed with clay, but they cannot, on any, ſuperſede its uſe*.

The whiteſt clays are eſteemed the pureſt, but even theſe, beſides other impurities, generally contain a very conſiderable portion of fine ſand. I took 16 ounces of the fineſt pipe clay from Dorſetſhire, and by repeatedly waſhing it in large quantities of water, and pouring off the turbid water, I collected a ſandy ſediment, amounting, when well dried, to three ounces. I have no doubt that this clay contained a much greater proportion of ſand, than what I had been able to collect; for the white particles which had been ſuſpended in the water, certainly conſiſted in part of a ſand of a finer grain, than what had ſettled to the bottom, for they were very ſenſibly gritty between the teeth. It may eaſily be conceived, that in waſhing clays, the fineſt part of the ſand contained in them will remain ſuſpended in the water, and that on this account, the ſediment collected at the bottom of the veſſel, in which the operation is performed, will not give the true proportion of the ſand which enters into their compoſition. In confirmation of this idea, we have been inſtructed by the experiments of a very able chemiſt, to conſider fine white clays, as conſiſting of about 3 parts in 8 of true argillaceous earth, and of about 5 parts in 8 of ſand, or earth, reſembling powdered flints*. All ſorts of earthen wares from the coarſeſt pots to the fineſt pieces of either Aſiatic or European porcelane, conſiſt either wholly or principally of clay. There are a great many ſorts of clay which differ from each other in colour, in tenacity, in the degree of contraction which they ſuffer in being burned, in their power of reſiſting the action of fire without being melted, and in many other properties; and hence we need be at no pains to prove, that the properties of earthen wares muſt be very various, according to the properties of the clays from which they are formed; and this variety becomes ſtill greater, when we take into conſideration the ingredients which are purpoſely mixed with the natural clays.

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* In the Mem. de l'Acad. des Scien. de Pruſſe, 1749, there are ſome experiments tending to prove, that the tenacity of clay is owing to ſome inflammable matter which is mixed with it, and which is eaſily conſumed in the fire.

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* M. Marggraf's Opuſ. Chy. Vol. II. p. 171.—Is it poſſible, by any artificial or natural comminution of its parts, to reduce the ſand found in clay into a true argillaceous earth?

The *flint* or *white-ſtone* ware is made in Staffordſhire, and other places, in the following manner.— Tobacco-pipe clay, which they have

from

from Dorſetſhire, is beat much in water; by this proceſs the finer parts of the clay remain ſuſpended in the water, whilſt the coarſer ſand and other impurities fall to the bottom. The thick liquid, conſiſting of water and the finer parts of the clay, is further purified by being paſſed through hair and lawn ſieves of different degrees of fineneſs: the clay is then ſufficiently prepared to be mixed with powdered flint, the other ingredient in the ſtone ware. They uſe annually in Staffordſhire about 5000 tons of flint, which they have from Hull. They have a tradition concerning the firſt introduction of flint into their potteries, which is briefly this:—A Staffordſhire potter, about 80 years ago, in travelling to London, met at an inn upon the road with an hoſtler, who undertook to cure a diſorder in his horſe's eye. The hoſtler took a flint ſtone, and having calcined it for a ſufficient length of time in a hot fire, it became very white; he pounded it into a fine powder, and blew ſome of it into the eye of the animal: the potter took the hint, conceiving that as flints could be calcined and pounded into a fine white powder, they might be uſed in conjunction with clay to make a whiter ware than the clay would do alone. He amaſſed large quantities of flints, burned and pounded them in ſecret, and found the event anſwerable to his expectation. The diſcovery ſoon became general, and many lives were loſt from the powder of the flint being inhaled by the men employed in pounding it: horſes were afterwards uſed for the ſame purpoſe, and for ſome time paſt they have been accuſtomed to grind their flint*. If the flints are pounded, or ground by inſtruments of iron, the powder is not ſo good as it ought to be, for the particles of the metal, which are abraded during the operation, being mixed with the powder, give the ware, when burned, a bad colour.

They formerly were accuſtomed to grind their flints with *Moorſtone*, or *Granite*; but of late years a large bed of *Chert* (a ſpecies of flint) has been diſcovered near *Bakewell* in *Derbyſhire*, and the Staffordſhire and Yorkſhire potters prefer it to Moorſtone for grinding their flints: it is very hard, and being itſelf of the nature of flint, the parts of it which are worn off and mixed with the flints in grinding, do not vitiate the quality of the flint powder. It is ſold at the quarries for 8s. a ton, of which the *Duke* of *Rutland*, as lord of the ſoil, has 5s. and the workmen the other 3s. for raiſing it. It is reckoned that they annually raiſe between four and five hundred tons, beſides the ſmaller pieces, which are thrown by as refuſe; but which might be calcined upon the ſpot inſtead of flint, and being ground there, might anſwer the purpoſe of ground flint; though as coals are comparatively dear at *Bakewell*, and as there is no navigation from thence into Staffordſhire, I am not certain whether the ground *Chert* could be afforded to the potters at an eaſier rate than they can prepare their flint. However, *Chert* is very commonly to be met with in other parts as well as at Bakewell, though not, perhaps, in ſuch large quantities, and therefore the hint of uſing calcined Chert, as a ſubſtitute for calcined flint, may be of ſervice to thoſe who are better ſituated with reſpect to fuel and water carriage, than they are at Bakewell.

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* I ſuſpect the date aſſigned by this tradition to the potter's diſcovery, for the *grinding* of flints is incidentally mentioned as practiſed at *Broſely* in the year 1697.—Phil. Tranſ. No. 228.

When the flints have been properly calcined and ground, they are ſifted in water till the water is, as near as may be, of the ſame thickneſs as that in which the clay is ſuſpended;

pended; then the liquid clay and flints are mixed together in various proportions, for various wares, and left to ſet; the mixture is then dried in a kiln, and being afterwards beaten to a proper temper, it becomes fit for being formed at the wheel, into diſhes, plates, bowls, &c. When this ware is to be put into the furnace to be baked, the ſeveral pieces of it are placed in caſes, made of clay, called *Seggars*, which are piled upon one another, in the dome of the furnace; a fire is then lighted, and when the ware is brought to a proper temper, which happens in about 48 hours, it is glazed by common ſalt. The ſalt is thrown into the furnace (through holes in the upper part of it), by the heat of which it is inſtantly converted into a thick vapour, which circulating through the furnace, enters every Seggar through holes made in its ſide (the top being covered, to prevent the ſalt from falling upon the ware), and attaching itſelf to the ſurface of the ware, it forms that vitreous coat upon the ſurface which is called its *glaze.*

This very curious method of glazing earthen ware, by the vapour of common ſalt, was introduced into England from Holland, at leaſt it was introduced from thence into Staffordſhire, about 80 years ago, by two Dutchmen. An old man informed the perſon from whom I had the account, that he remembered, when he was a boy, running with others to help to extinguiſh what from the ſmoke they apprehended to be a fire in the pottery where the Dutchmen were working, but that their entrance was oppoſed by the proprietors of the pottery, who were unwilling that the cauſe of the ſmoke, which was the common ſalt they were uſing in glazing their ware, ſhould be generally known.

The *Yellow* or *Queen's ware*, is made of the ſame materials as the *flint* ware, but the proportion, in which the materials are mixed, is not the ſame, nor is the ware glazed in the ſame way. The flint ware is generally made of 4 meaſures of liquid flint, and of 18 of liquid clay; the yellow ware has a greater proportion of clay in it; in ſome manufactories they mix 20, and in others 24 meaſures of clay with 4 of flint. Theſe proportions, if eſtimated by the

weight

weight of the materials, would probably give for the flint ware, about 3 hundred weight of clay to 1 hundred weight of flint, and for the yellow ware ſomewhat more clay. The proportion, however, for both ſorts of ware, depends very much upon the nature of the clay, which is very variable even in the ſame pit: hence they cannot be certain of the exact quantity of flint, which they ought to mix with the clay, till they have made ſome trial of the quality of the clay by burning a kiln of the ware. If there is too much flint mixed with the clay, the ware, when expoſed to the air after burning, is apt to crack; and if there is too little, the ware will not receive the proper glaze from the circulation of the ſalt vapour. This glaze, even when it is moſt perfect, is an appearance leſs beautiful than the glaze on the yellow ware. This yellow glaze is made by mixing together in water, till it becomes as thick as cream, 112 pounds of white lead, 24 pounds of ground flint, and 6 pounds of ground flint glaſs. Some manufacturers leave out the glaſs, and mix only 80 pounds of white lead with 20 pounds of ground flint, and others doubtleſs obſerve different rules, of which it is very difficult to obtain an account. The ware, before it is glazed, is baked in the fire, by this means it acquires a property of ſtrongly imbibing moiſture (we perceive ſomething of this in the force with which a new pipe ſticks to the lips, though the pipe has received a kind of glaze by being rubbed with bees wax after being baked), it is therefore dipped in the liquid glaze and ſuddenly taken out, the glaze is imbibed into its pores, and the ware preſently becomes dry: it is then expoſed a ſecond time to the fire, by which means the glaze, which it had imbibed, is melted, and a thin glaſſy coat is formed upon its ſurface: the colour of this coat is more or leſs yellow, according as a greater or leſs proportion of lead has been uſed. The lead is principally inſtrumental in producing the glaze, as well as in giving it the yellow colour; for lead, of all the ſubſtances hitherto known, has the greateſt power of promoting the vitrification of the ſubſtances with which it is mixed. The flint ſerves to give a conſiſtency to the lead during the time of its vitrification,

tion, and to hinder it from becoming too fluid, and running down the ſides of the ware, and thereby leaving them unglazed. The yellowiſh colour, which lead gives when vitrified with flints, may be wholly changed by very ſmall additions of other mineral ſubſtances; thus, to give one inſtance, the beautiful *black glaze*, which is fixed on one ſort of the ware made at *Nottingham*, is compoſed of 21 parts by weight of white lead, of 5 of powdered flints, and of 3 of an earthy, or metallic mineral called *Mangane∫e*.

The powder into which calcined flints are ground, is whiter than common ſand, and of a finer grain; and hence it conſtitutes, when mixed with clay, a ſpecies of earthen ware, ſuperior to that in which ſand is uſed as an ingredient; the coarſe *ſtone ware* made at *Briſtol*, is compoſed of tobacco-pipe clay and ſand, and is glazed by the vapour of ſalt, like the Staffordſhire flint ware, but it is far inferior to it in beauty. As our flint ware is ſuperior to the ſtone ware here mentioned, becauſe it is made of materials, which, though of the ſame kind, are of a finer quality, than thoſe which enter into the compoſition of ſtone ware; ſo is it inferior to all the ſorts of porcelane in whiteneſs and tranſparency; becauſe powdered flints and tobacco-pipe clay, are neither of them ſo white and pure as the clays and flints of which all porcelanes are made. According to this notion, the only difference between porcelanes and many ſorts of earthen ware, conſiſts in the fineneſs and proportion of the ingredients; and theſe cauſes joined to the different degrees of heat employed in baking different porcelanes, are abundantly ſufficient to account for all the varieties obſervable in the grain, colour, hardneſs, and tranſparency, of different ſorts of china.

We are told that the component parts of oriental porcelane, "are (1) a *vitreſcent* ſtony ſubſtance, reduced to an impalpable powder, *Petunſe*. (2) An *unvitrifiable* unctuous clay, reduced to an impalpable powder alſo, *Kaolin*. Theſe are thoroughly mixed, the latter in as ſmall proportion as poſſible, and then moulded and baked. (3) A varniſh compoſed of the fineſt particles of a ſoapy earth, diſſolved in water to the conſiſtence of a cream, in which, when dry, the pieces are dipped before they are baked *." The Chineſe *Petunſe* differs from powdered flints, in being fuſible in a ſtrong degree of heat, whereas flints are not fuſible in a fire ſufficient for the baking of porcelane, unleſs they are mixed with ſome heterogeneous matter; and this property of the *Petunſe* probably gives to china, a degree of tranſparency not obſervable in our flint ware; for the vitrifiable *Petunſe*, when mixed with the unvitrifiable clay, may communicate a degree of vitreouſneſs to the compound.

The moorſtone or *granite* found in *Cornwall* is thought to reſemble the Chineſe *Petunſe*; and the *Soap-rock*, which is plentifully met with at the *Land's End*, is eſteemed a ſpecies of *Kaolin*,

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* Campbell's Surv. of Brit. Vol. II. p. 18.

Kaolin, and is much ſought after by the makers of porcelane. The granite is looked upon by mineralogiſts as a vitrifiable ſtone; at leaſt, as to the greateſt part of it *, and there may, probably, be diſcovered other ſtones which poſſeſs the ſame property. The lead mines in Derbyſhire abound in two kinds of ſpar, which are very different from each other in figure (where the figure is diſcernible), in weight, and in the change which they undergo from the action of fire. The figure of one of the kinds of ſpar is that of a *lozenge* or *rhomb*; the figure of the other is that of a *die* or *cube*: a cubic foot of the *rhomboidal*

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* ——— Expoſé au feu, il s'y vitrifie, à l' exception du *mica*, et du cement qui ſouffrent la meme violence du feu ſans en etrê altéré.—Miner. par M. Val. de Bomare, Vol. I. p. 275.

boidal ſpar weighs 2700 ounces; a cubic foot of the *cubical* ſpar weighs 3219 ounces: the *rhomboidal* ſpar, in being calcined in a ſtrong fire, loſes near 18 parts in 40 of its weight, and is changed into quicklime; the *cubical* ſpar in the ſame degree of heat loſes about 1 part in 40 of its weight, and is not converted into quicklime: the *cubical* ſpar may be melted in a degree of heat, in which the *rhomboidal* ſpar remains unmelted; and from this property, the cubical ſpar might, probably, be uſed with advantage by the porcelane manufacturers. Some mines, as thoſe near *Critch* in *Derbyſhire*, yield no kind of ſpar but the cubical; whilſt others yield only the rhomboidal; and ſome yield both: I have ſeen a narrow crack in a limeſtone rock, coated on one ſide with the cubical, and on the other with the rhomboidal ſpar.

Having fallen into the mention of ſpars, it may juſt be obſerved, that *Blue-John*, which has, of late years, been fabricated into vaſes, and other ornamental figures, is of the ſame quality as the cubical ſpar, with reſpect to its fuſibility in the fire; it loſes, moreover, its colour, and becomes white in a moderate heat: the weight of a cubic foot of the blueſt kind is 3180 ounces, and that of a cubic foot of the leaſt blue is 3140 ounces. This ſubſtance was firſt diſcovered (or more properly ſpeaking, it firſt began to be applied to ſome uſe), about 16 years ago, at one of the oldeſt lead mines in *Derbyſhire*, called the *Odin* mine (probably from its being dedicated to *Odin*, the great God of the northern nations), at the foot of a high mountain called *Mam Tor* near *Caſtleton*. Spars tinctured with the *amethyſt* colour have been diſcovered in other mines; there is great plenty of what they call the *chocolate* coloured ſpar, in a mine at *Critch* (a few miles from *Matlock)*, where I ſaw the laſt year, a piece of oak, which had ſtood long in the water of that mine, tinged with a deep purple colour; but the greateſt quantity of *Blue-John* is ſtill raiſed at *Caſtleton*; the largeſt pieces are ſold for 9 pounds a ton, the middle-ſized for 6 pounds, and the leaſt for fifty ſhillings.—But to return to the conſideration of porcelane.

Several European nations, in particular the *Germans*, the *Engliſh*, the

French, and the *Italians*, have, ſince the beginning of the preſent century, applied themſelves with great induſtry and ingenuity, to the making of porcelane, and they have all of them arrived at great perfection in the art; though none of the European porcelanes, unleſs, perhaps, we except the *Dreſden*, can yet be ſaid to equal, both in ſtrength and beauty, the porcelanes which are imported from *China* and *Japan*. The Dreſden, and the German porcelanes in general, are as unvitrifiable as the Aſiatic; while moſt of the others which are made in Europe, may be vitrified by a longer continuation of the heat, in which they are baked. The being or the not being convertible into glaſs in a ſtrong fire, is one of the principal criterions by which connoiſſeurs judge of the goodneſs of porcelane, that being eſteemed the beſt which is the leaſt fuſible. Porcelane being ſeldom expoſed to a degree of heat, greater than that of boiling water, it ſeems to be an odd method of judging of its worth, by inquiring whether it is capable of ſuſtaining, unaltered in its form, a degree of heat ſufficient to convert many other ſubſtances into glaſs; but probably that ſort of it is the moſt hard, and tough, and beſt capable of bearing the ſudden tranſitions from heat to cold, which is baked with the ſtrongeſt heat; and, if this be true, then there is a good reaſon for eſteeming that china to be the beſt, which is the moſt unfuſible, inaſmuch as it has, probably, undergone a ſtronger fire in being baked than the more fuſible kinds could ſupport.

There is ſome reaſon for thinking, that the ſtrength of porcelane has ſome dependence on its weight, for the more cloſely its parts are compacted together, the more forcibly, other circumſtances being the ſame, will they reſiſt the impulſe of a ſtroke, tending to diſunite them. I have eſtimated the relative weights of moſt of our Engliſh porcelanes; but I forbear mentioning particulars, as I ſhould be ſorry to ſay any thing which might have the moſt diſtant tendency to injure any individual; eſpecially as the great price of labour in this country, and, indeed, in all parts of Europe, compared with its price in Aſia, muſt ever render the engaging in a porcelane manufactory a hazardous undertaking whilſt the importation of Aſiatic porcelane is allowed. The beſt mode of encouraging the European manufactories of porcelane, would be the impoſition of a heavy duty on that which is brought from Aſia. It is, I believe, generally acknowledged, that the *yellow ware*, is far more brittle than the *flint ware*; the relative weights of a cubic foot of theſe wares and of Eaſt India china, were

Eaſt India china	2346	ounces avoir.
Flint ware	2188	
Yellow ware	1988	

Now ſuppoſing a plate of the common yellow or Queen's ware (for there are very different ſorts of it) to weigh 12 ounces; then will a plate of the flint ware of exactly the

ſame

ame ſize and thickneſs weigh $13\frac{2}{5}$ ounces; and an equal plate of china will weigh $14\frac{1}{6}$ ounces; and hence we need not wonder at the great fragility of the common yellow ware, when compared either with china or ſlint ware. The coarſe Briſtol ſtoneware has nearly the ſame weight as china, a cubic foot of it weighing 2340 ounces.

The preciſe meaning of the term *marle* is not yet fully fixed; it is moſt generally uſed to denote an earthy or ſtony ſubſtance, conſiſting principally of *clay* and *calcareous* earth, and falling into powder by expoſure to the air. There are ſeveral ſolid ſtrata of ſtones, ſuch as the Derbyſhire *ſhale* and *toadſtone*, which moulder into powder by expoſure to the air, and yet they are not eſteemed marles, becauſe they do not contain any calcareous earth; there are alſo limeſtones which contain portions of clay, and clays which contain portions of limeſtone, but which not falling into powder by the influence of the air, are not claſſed amongſt the marles: and hence theſe two properties, of containing calcareous earth, and of falling into powder when expoſed to the air, may be eſteemed characteriſtic of marles: and they are, perhaps, the characteriſtics which are moſt to be relied on, for as to colour and conſiſtency, marles cannot by them alone be diſtinguiſhed from ſeveral other bodies.

As the uſe of marle in agriculture, depends in ſome ſort of ſoils upon the quantity of calcareous earth which it contains, and in others upon the clay which enters into its compoſition, ſome readers will wiſh to be acquainted with a general method of aſcertaining the exact proportion of clay and calcareous earth, contained in any particular kind of marle.

Take an ounce of the propoſed marle, previouſly reduced into powder, and well dried; pour upon the marle a diluted acid of ſea ſalt; an effervefcence will enſue; this effervefcence ariſes from the action of the acid on the calcareous earth, for the acid has no ſuch action on pure clay, nor even on clay and ſand, as may be ſeen by pouring a little of it on pipe-clay; when ſo much acid has been added to the marle that no further effervefcence is obſerved, we may conclude that all the calcareous earth contained in the marle is now diſſolved in the acid: put the whole into a filter; the calcareous earth being diſſolved in the acid will paſs through the filter, whilſt the clay will remain upon the filter: waſh the clay, and the filtering paper, by pouring upon them hot water till they are freed from all ſaline particles; the clay being afterwards dried, as the ounce of marle had been, will ſhew, by its loſs of weight, the quantity of calcareous earth which the marle contained. As to the matter which remains upon the filter, and which has been called clay, it may chance either to be pure clay, or a mixture of clay and ſand; if it is the latter, the quantity of ſand may be known by waſhing it and collecting the ſediment.

This method of examining how much calcareous earth is contained in any ſpecies of marle, may be uſefully adopted alſo when the clay or marle is to be applied to the making of brick. For if the quantity of calcareous earth is large, the marle in being burned will rather form lime than brick, or the brick at leaſt will ſoon moulder in the air, in conſequence of the calcareous earth falling into a powder like lime.—The marle dug out of the ſame pit is often very different in its properties, ſome parts of it containing ſo much calcareous earth, that they may with advantage be burned into lime; whilſt other portions of it may be made into good brick. I will

illuſtrate

illuſtrate what has been ſaid concerning the analyſis of marle, by an inſtance.

We have a ſtratum of lead-coloured clay, a little beyond Magdalen College, on the road to Ely, which burns to a whitiſh brick: I took four ounces of this clay, moiſt as it was dug out of a pit, ten or twelve feet deep; by being dried on a hot fire-ſhovel till no more vapour appeared to riſe from it, the weight was reduced to three ounces. Theſe three ounces, or 60 pennyweights, were put into a quantity of diluted acid of ſalt, till no more efferveſcence was obſerved, the mixture was filtered, and what remained upon the filter, being dried, weighed 46 pennyweights, the quantity of calcareous earth amounting to 14 parts in 60, or near $\frac{1}{4}$ of the weight of the clay when dried. What remained upon the filter was waſhed in much water, I obtained a ſediment amounting to about 1 pennyweight, but it was not a ſandy ſediment, it reſembled an earthy ſalt, reſulting from an union of the acid of vitriol with calcareous earth, which is uſually called *Selenites*; there was alſo ſeparated, by waſhing, a quantity of greyiſh earth, which was chiefly iron.

The moulds, uſed in making bricks from this clay, are 10 inches in length, and 5 in breadth, and the bricks, when burned, uſually meaſure 9 inches in length, and 4½ in breadth, ſo that the clay ſhrinks about 1 inch in 10. But the degree of contraction, which clays undergo in being burned, is very various, and it does not ſolely depend upon the purity of the clay; for I have been informed by perſons employed in making coarſe earthen ware from ſimple clay, that the ſame clay juſt dug out of the earth, and put into the fire, will contract leſs, than when it has been ſome time expoſed to the influence of the atmoſphere; for froſt more perfectly divides its particles than any artificial proceſs can do: the Chineſe are ſaid to let their clay remain expoſed to the atmoſphere 20 or 30 years, before they apply it to the making of porcelane: and the goodneſs of common tiles, and bricks, is much increaſed, by ſuffering the clay to lie mellowing for a year or two before it is uſed.

The clay here ſpoken of, though it does not contain ſo much calcareous earth, as to render it a proper manure for ſtiff, loamy ſoils, yet it might probably be uſed, with great advantage, on thoſe which are light and ſandy; inaſmuch as by its clayey part (for clay is greatly retentive of humidity) it would contribute to keep the ground from being depriv-ed of its moiſture, by the action of the ſun and wind during the height of ſummer. The quantity of moiſture which is imbibed by the leaves of vegetables from the atmoſphere, as well as that which is ſucked in by their roots, has a great dependence on the moiſture of the ground on which they grow; for when the ground is dry, the air incumbent upon it will, comparatively ſpeaking, be dry alſo, and the vegetables which grow in that dry air, will lack a part of that moiſture which is neceſſary to their well being, for whether *moiſture* be itſelf the *food* of vegetables, or only the *vehicle* of food, it is indiſpenſably neceſſary for the promotion of vegetation *.

From the conſideration of marle we paſs to that of *alabaſter*, which is ſeldom found in beds, except in conjunction with marle. There is ſome confuſion both in the philoſophical and in the common acceptation of the term *alabaſter*, I do not mean merely with reſpect to the colour and tranſparency of the ſubſtance, but with relation to its conſtituent parts. This confuſion may be greatly removed, by admitting two ſorts of alabaſter, which are eſſentially different from each other, *calcareous* and *gypſeous* alabaſter: to which ſort any particular ſpecimen belongs may be determined by the eaſieſt experiment. Dip a feather in the acid of ſea ſalt, touch the ſpecimen of alabaſter with the wet feather, if an *efferveſcence* enſues, the alabaſter is *calcareous*, it is of the nature of marble, and may be burned into lime; if no *efferveſcence* enſues, the alabaſter is *gypſeous*, it is of the nature of *Plaſter-of-Paris*, and very different from every ſort of marble. This ſubſtance is called in the north of England *allplaſter*, being a corruption probably of alabaſter, and it is alſo often called by the Latin appellation *gypſum*, eſpecially by the

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* Vegetabilia omnia a liquoribus omnino creſcunt.—Newton.

French. *Gypſum* was applied by the ancients to the very ſame purpoſes to which it is applied by us; they uſed it in architecture for the making of ceilings, medallions, and cornices; and even the name of the perſon, who firſt invented the method of taking off the impreſſion of a man's face in gypſum, has been preſerved by Pliny *.

There are ſome ſorts of *alabaſter* which differ ſo widely in weight, both from the fineſt kinds of plaſter-ſtone, and of calcareous ſtone, that I know not how they can properly be conſidered as being of the ſame nature with either of them. Mr. Cotes has fixed the weight of a cubic foot of alabaſter at 1875 ounces[*], and two other authors have repreſented it as weighing 1872 ounces[†]. I have tried ſeveral ſpecimens of the fineſt Derbyſhire plaſter-ſtone, and find that a cubic foot of it weighs, at a medium, 2286 ounces; a cubic foot of the Derbyſhire watricle, which is a fine calcareous ſtone, reſembling alabaſter in colour and tranſparency, weighs 2720 ounces.

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* Uſus *gypſi* in albariis, ſigillis ædificiorum, et coronis gratiſſimus. Plin. Hiſt. Nat. L. xxxvi. ſ. 59.—Hominis autem imaginem *gypſo e facie ipſa* primus omnium expreſſit Lyſiſtratus. Id. L. xxxv. ſ. 54. where it is obſerved that the *caſt* was improved by pouring wax into the gypſum mould.

Two ſubſtances when combined together in different ways, or in different proportions, may conſtitute a great variety of bodies differing much from each other in external appearance, and ſomewhat, probably, in their internal properties. We have an inſtance of this in the combination of the acid of vitriol with calcareous earth, for this combination conſtitutes a great many ſubſtances exceedingly different from each other in appearance. I will mention ſome of the moſt remarkable. (1) A ſolid ſubſtance not ſo hard nor ſo heavy as marble, nor ſo capable of receiving a fine poliſh, but conſiſting of finer particles, and having a greater degree of tranſparency — *Gypſeous alabaſter.* (2) A ſubſtance compoſed of larger particles than the preceding, and having a ſcaly or granulated texture— *Plaſter-ſtone.* (3) A ſubſtance whoſe parts, inſtead of being flat or granulated,

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[*] Cotes's Hydros.

[†] Muſſchenbroek—Wallerius.

lated, lie in longitudinal fibres, like a parcel of needles, or a ſkein of thread — *Striated gypſum.* (4) A pellucid ſubſtance conſiſting of a number of tranſparent plates of a lozenge-like form, found in great plenty and perfection at *Shotover-hill*, near Oxford—*Rhomboidal ſelenites.* (5) Some take the *lapis ſpecularis* which the ancients uſed inſtead of glaſs, to have been compoſed, like all the preceding bodies, of the acid of vitriol and calcareous earth; whilſt others look upon it as a ſpecies of *talc*, which differs from plaſter-ſtone in this, that it does not, after being calcined and wetted with water, ſwell and concrete into a hard ſtony ſubſtance. This *lapis ſpecularis* came into uſe at Rome in the age of *Se-*

neca[*]; it was ſoon after its introduction applied not only to lighten apartments, but to protect fruit trees from the ſeverity of the weather[†]; and it is recorded to have been principally by its means, that the Emperor Tiberius was enabled to have cucumbers at his table during almoſt every month in the year[‡]. I believe it is ſtill uſed in ſome countries in the place of glaſs, and we certainly know that it was ſo uſed in the time of *Agricola*, for he mentions two churches in Saxony which were lighted by it *.

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[*] Quædam noſtra demum prodiiſſe memoria ſcimus, ut *ſpeculariorum* uſum, perlucent teſta, claram tranſmittentium lucem. Sen. E. p. 90.

[†] Pallida ne Cilicum timeant pomaria brumam,
Mordeat et tenerum fortior aura nemus:
Hibernis objecta notis *ſpecularia* puros
Admittunt ſoles, et ſine fæce diem.
Mar. L. viii. epig. 14. et 68.

[‡] Nihilo minus *ſpecularibus* integi debebunt ut etiam frigoribus ſerenis diebus tuto producantur ad ſolem. Hac ratione fere toto anno Tiberio Cæſari *cucumis* præbebatur. Colum. L. xi. c. 3.

If the *lapis ſpecularis* was a kind of tranſparent *plaſter-ſtone*, it muſt have been ſubject to one inconvenience, it could not have long ſtood the action of the weather: for *gypſeous alabaſter*, *plaſter-ſtone*, *ſelenites*, and all other combinations of the *acid* of *vitriol* with *calcareous earth*, are ſoluble in water, either wholly or in a great degree. Theſe ſubſtances require, indeed, a greater proportion of water to diſſolve them, than any other ſaline ſubſtances do, but they are, nevertheleſs, ſoluble in water.

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* Agric. de Natura Foſ. L. v. p. 257.

I took ſome very white plaſter-ſtone, and pounded it into a fine powder; 1 pennyweight of this powder was put into a pail, containing 10 quarts of water; the powder was entirely diſſolved in it; from hence we learn, that this kind of plaſter-ſtone is wholly ſoluble in water; the water was pump water of 48 degrees of heat. I thought that this quantity of water was perfectly ſaturated with 4 pennyweights, or 96 grains of the powder, and hence it ſeems as if ſpring water, which is uſually of the temperature of 48 degrees, would diſſolve about one fourteen hundredth part of its weight of plaſter-ſtone. There are great beds of plaſter-ſtone in the environs of Paris, and the hardneſs of the well water of that city is thought to ariſe from the particles of plaſter-ſtone which are diſſolved in it.

Agricola eſteemed the *lapis ſpecularis* to have been a ſpecies of plaſter-ſtone, and in ſpeaking of it he remarks, that though it could bear, without being injured, the heat of ſummer and the cold of winter, yet, the largeſt maſſes of it were waſted by the rain*. I tied a lump of plaſter-ſtone, weighing two ounces, to the end of a pair of ſcales, and kept it ſuſpended in the middle of a pail of water for 48 hours, the water having been changed two or three times in the interval; at the end of the experiment, it appeared, that the plaſter-ſtone had loſt one thirtieth of its weight.

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* Res *gypſo* cognata *lapis ſpecularis*—fert calores ſolis et hyemis frigora, ſed imbrium adeo impatiens eſt, ut ſi vel magnæ ejus moles ipſis fuerint expoſitæ corrumpantur. Agric. de Nat. Foſſ. L. v. Ed. Baſ. 1546.

There are many places in England, where plaſter-ſtone is gotten in great abundance, the principal of thoſe which I have ſeen, are at *Chelaſton*, a ſmall village about four miles ſouth from *Derby*, and at *Beacon-Hill* near *Newark*. They annually raiſe at Chelaſton about 800 tons of plaſter-ſtone, which is ſold upon the ſpot for 5s. a ton. It is chiefly uſed for the making of floors in Derbyſhire, Leiceſterſhire, and Nottinghamſhire: that which has a blueiſh caſt is moſt eſteemed for flooring; the very white pieces are ſold to the potters; to the poliſhers of ſpar, who call it alabaſter (though they get their beſt ſort of alabaſter from Staffordſhire); and to the plaſterers. The plaſter-ſtone before it is uſed, either for floors or ceilings, is calcined; it loſes in that operation, from 4 to 6 hundred weight in a ton; the loſs, however, in the ſame kind of ſtone, depends very much upon the degree of heat with which it is calcined. I calcined, in the ſame crucible, a piece of Derbyſhire watricle, a piece of fine grained plaſter-ſtone, and a piece of the ſtriated kind from Chelaſton. The watricle loſt after the rate of 8 c. 3 q. 17 lb. in a ton; the plaſter-ſtone loſt after the rate of 12 c. 0 q. 3 lb. and the ſtriated kind after that of 10 c. 0 q. 2 lb. in a ton. When the ſtone is calcined, thoſe parts of it which are expoſed to the greateſt heat, are ſometimes melted, and they frequently acquire a yellowiſh caſt, and emit a ſulphureous exhalation, and are in both caſes thrown aſide as uſeleſs. The flooring made from the ſtones, which are become ſulphureous by calcination, is apt to riſe in bliſters; this proceeds from the plaſter-ſtone having loſt a portion of the vitriolic acid, which enters into its compoſition, and being thereby changed into a ſpecies of lime. They calcine the ſtone uſually at night, that they may be the better able to obſerve when it becomes red hot, in which ſtate they judge it to have received heat enough. After calcination they reduce it to a powder, by threſhing it with flails, or grinding it in a mill; and when it is become of a due fineneſs, they mix it with a proper quantity of water, and ſpread it to the required thickneſs upon reeds, where it immediately concretes into a ſolid floor; 30 hundred weight of the crude ſtone will, when calcined, make 20 ſquare yards of flooring of 2½ inches in thickneſs. For preparing the ſtone, and ſhooting the floor, the Derbyſhire maſons charge 9d. a ſquare yard; hence 20 ſquare yards of plaſter flooring, including the price of the ſtone at the pits, will coſt 1 l. 2 s 6 d.——The additional expence muſt be various, it depending upon the price of the conveyance of the crude ſtone, from the place where it is dug, to the place where it is uſed. Plaſter-ſtone, however, is found in ſo many parts of the kingdom, and the inland navigations are become ſo commodious, that plaſter flooring might be introduced into many countries with a conſiderable ſaving of expence; a circumſtance eſpecially to be attended to in the erection of public infirmaries, and houſes of induſtry.

They have ſawn out of the quarry at Chelaſton, ſquare blocks of a yard in breadth and thickneſs, and of two yards in length; and blocks of ſtill greater dimenſions have been raiſed at a place called *Red-hills*, in the county of Nottingham, about four miles from Chelaſton. The ſtone from the *red-hills* is not ſo good for flooring, as that at Chelaſton; it is, however, a very beautiful ſtone, *Lord Scarſdale* having gotten from thence thoſe ſuperb columns which adorn his hall at *Kedleſton*. Theſe columns are made of different blocks, from 2 or 3, to 6 or 8 feet in height, which are ſo artfully cemented together, that they may be taken for one piece. The blocks are raiſed out of the quarry, by being ſawn to the proper length on each ſide, and to the depth requiſite for the thickneſs of the column, the block ſtill adhering by its lower ſurface, to the quarry, is broken off from thence by being raiſed on each ſide with levers. It is afterwards rounded into the proper cylindric form by the mallet and chiſel, and poliſhed by being rubbed with various kinds of ſtones, then with emery, and laſt of all with putty. If the veins of the ſeparate blocks which are to compoſe the ſhaft of an alabaſter column, of the kind we are ſpeaking of, do not happen to ſuit each other; in order to make the juncture leſs diſcernible, and to give a greater appearance

of

of conſiſtence to the whole column, ſmall pieces are here and there cut out of the ſeparate blocks, and others of a different colour inſerted, and cemented by a mixture of roſin and bees-wax, tinged green, red, brown, yellow, according to the nature of the piece to be inſerted.

Alabaſter columns, though they are much eaſier to be wrought than marble, and when finely poliſhed look very well, yet, I apprehend, their poliſh muſt be injured by a circumſtance which does not affect either *marble* or *calcareous alabaſter*. The atmoſphere is always much charged with humidity, upon the breaking up of a froſt, and in other ſtates of the weather, ſo much water is diſcharged from the air, that marble, wainſcot and other ſubſtances upon which it ſettles, are ſaid to ſweat.— Now the water which thus ſettles on a *gypſeous alabaſter* column, will diſſolve part of the ſubſtance of the column, and as ſome parts of the alabaſter, are more ſoluble than others, the uniformity of its ſurface will be injured; and the part, moreover, of the alabaſter which has been diſſolved, will, upon the evaporation of the water, either not adhere at all to the column, or it will adhere in ſuch a way, as to render the poliſh leſs gloſſy. But whether this account of the matter be admitted or not, the fact, I believe, of the poliſh of gypſeous alabaſter being injured by moiſt weather, is not to be denied. The alabaſter columns in the Egyptian Hall at *Holkam*, were dug from the quarries in Staffordſhire, and the late Lord *Leiceſter* was at the trouble of contriving means to keep them dry in moiſt weather, leſt their poliſh ſhould be impaired by the action of the moiſture.

They annually raiſe about 2000 tons of plaſter-ſtone at the pits near Newark; it is ſold on the ſpot at 5s. 6d. and at Gainſborough at 8s. 6d. a ton, from whence it is ſent to London and various other places.

It having been ſaid, that plaſter-ſtone is a ſaline ſubſtance reſulting from an union of the acid of vitriol with a calcareous earth, the reader may expect ſome proof of the aſſertion.—— Boil together pot-aſh and plaſter-ſtone, and you will obtain the ſame kind of ſalt, which would ariſe from uniting the acid of vitriol with pot-aſh; and thence we conclude, that the *acid of vitriol* is one of the component parts of plaſter-ſtone: that *calcareous earth* is the other, is evident from this conſideration, that the earth, which remains after the acid of vitriol has been ſeparated from the plaſter-ſtone, effervеſces with acids, and may be burned into lime.

Having ſaid ſo much concerning gypſeous alabaſter, which is an earthy ſalt reſulting from an union of the *acid of vitriol* with a *calcareous* earth; it may not be improper to add a word or two concerning *alum*, which is an earthy ſalt, compoſed of the *acid of vitriol* and an *argillaceous* earth. The acid of vitriol when united with any earth, may be ſeparated therefrom by a fixed alkali; and if we pour into a ſolution of alum any fixed alkali, the earth of alum will be precipitated, and we ſhall obtain a ſalt, of the ſame kind as would reſult from a direct combination of that alkali with the acid of vitriol; and thence it is inferred, that the *acid of vitriol* is one of the component parts of alum: upon examining the earth, which is precipitated by the alkali, it will be found to be very white, and very tenacious, and to be capable of being hardened in the fire; in ſhort, it is one of the pureſt clays in nature: and thence it is concluded that an *argillaceous* earth is the other component part of alum. The conſtituent parts of alum may alſo be proved by its artificial compoſition, for though the vitriolic acid, when poured upon pipe-clay, does not ſeem to act upon it, yet if it be boiled with it, a ſolution of the clay in the acid takes place, and an alum may be obtained by ſlowly evaporating the ſolution.

Being one day engaged in evaporating ſomething or other, from a ſaucer made of the Staffordſhire yellow ware, I was ſurpriſed to ſee a white ſubſtance bubbling through a crack in the ſaucer; upon taſting it I found it to be a ſalt; I collected a ſufficient quantity of it, and when it was diſſolved and cryſtallized, it appeared to be a perfect alum. The fire was a very hot one, and it was made of coal cinders; the ſaucer was placed on the bar of the grate, and the alum, I conceive, was formed from the *ſulphureous acid* of the cinders uniting itſelf with the *clay*, which enters into the compoſition of the yellow ware. I have been informed by a very intelligent perſon, that the vapour which, in ſome places, eſcapes from the coal-pits which are on fire in Staffordſhire, forms an alum whereever it meets with an argillaceous earth. This is very conformable to the manner of making alum on the *Solfatara* near Naples, where they place little heaps of *argillaceous* earths, or ſtones, over the crevices from which the ſulphureous vapour iſſues, in order that they may collect a greater quantity of alum*.

In England we make alum from a black ſlaty ſubſtance, called *ſhale*, this ſubſtance is a clay, hardened into a ſtony conſiſtence, and ſo much im-

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* Lett. ſur la Minera. par M. Feiber, French Tranſ. p. 261.

impregnated with bitumen, that it burns ſomewhat like coal. We have ſeen, that coal emits, when it is in the ſtate of a red-hot cinder, a ſulphureous acid: many other bitumens, probably, in the ſame ſtate emit the ſame acid, and the acid which is emitted from ſhale during its calcination, uniting itſelf to the argillaceous earth of the ſhale, forms alum. About 120 tons of calcined ſhale, will make 1 ton of alum. The ſhale, after being calcined, is ſteeped in water, by which means the alum, which is formed during the calcination of the ſhale, is diſſolved; this diſſolved alum undergoes various operations, before it is formed into the alum of the ſhops. The greateſt alum works, and, I believe the only ones which now ſubſiſt in the kingdom, are at *Whitby*; a few years ago there were ſome in Lancaſhire; and alum might be made in many other parts; but Whitby is ſo conveniently ſituated for coal and water carriage, that alum can be there afforded at a cheaper rate than in moſt other places, and it coſts even there 9 or 10l. a ton making.

ESSAY VIII.

OF PIT-COAL.

I Took 96 ounces of *Newcaſtle* coal, and putting them into an earthen retort, diſtilled them with a fire gradually augmented till nothing more could be obtained from them. During the diſtillation there was frequent occaſion to give vent to an elaſtic vapour, which would otherwiſe have burſt the veſſels employed in the operation. The weights of the liquid found in the receiver, and of the reſiduum remaining in the retort, after the diſtillation was finiſhed, were accurately taken, and are expreſſed in the following table.

Weight of *Newcaſtle* pit-coal diſtilled - - - 96 ounces

Weights of the products.	Liquid -	12	ounces.
	Reſiduum -	56	
	Loſs of weight	28	
		96	

Profeſſor *Newmann* analyſed, by diſtillation, the beſt ſort of the pit-coal found at *Halle*, in *Germany*. He uſed only 48 ounces in his analyſis, but I have expreſſed the reſult of his operation, upon the ſuppoſition of his having uſed twice as much, or 96 ounces*.

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* Newmann's Chem. by Lewis, p. 245.

Weight of the *Halle* pit-coal diſtilled - - 96 ounces

Weights of the products.	Liquid -	12	ounces.
	Reſiduum -	83¾	
	Loſs of weight	0¼	
		96	

Dr. *Venel*, of Montpellier, analyſed, by diſtillation, 64 ounces of the pit-coal of *Alais* in *Languedcc*; ſuppoſing that inſtead of 64 he had uſed 96 ounces, the reſult of his analyſis would have been expreſſed by the following numbers*:

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* Inſtructions ſur l'Uſage de la Houille. A. Avignon, 1775, p. 45.

Weight of *Alais* pit-coal diſtilled - - - 96 ounces.

Weights of the products.	Liquid -	10½ +	ounces.
	Reſiduum -	84	
	Loſs of weight	1½ —	
		96	

M. *Sage*, of the Academy of Sciences at Paris, ſays that he has diſtilled various ſpecimens of pit-coal, taken from different places; and that they generally yielded 2 ounces of liquid from 16 ounces of coal*, that is 12 ounces from 96; which is the ſame proportion as in the analyſis of the Newcaſtle and Halle pit-coal. He does not mention the weights of the reſiduums, ſo that the loſſes of weight, in his diſtillations, cannot be aſcertained.

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* Elémens de Mineralogie. A Paris, 1778, tome prem. p. 94.

Theſe ſeveral analyſes correſpond ſo nearly, with reſpect to the quantity of liquid procurable from different ſorts of coal, that they very much confirm one another; and make it probable, that we ſhall not be far from the truth in generally aſſerting, that pit-coal contains one-eighth part of its weight of liquid.

With reſpect to the nature of the liquid, it may be obſerved, that when poured out of the receiver, it is ſeen to conſiſt of three different ſubſtances: (1) The middle part is an acid watery fluid, of a reddiſh colour, it ſeems as if it was loaded with oil, and it ſmells of volatile alkali: (2) On the ſurface of this watery fluid there floats a ſmall portion

tion of oil, more or leſs liquid and tranſparent, according as the heat uſed in conducting the diſtillation has been leſs or greater: (3) At the bottom there is found a black thick tenacious oil, very much reſembling tar. The two oils together amounted in the diſtillation of the Newcaſtle coal to four ounces, or one-third of the weight of the whole liquid; in the analyſis of the Halle and Alais coals, as well as in the experiments of M. Sage, the two different ſorts of oils were obſervable, but they bore a greater proportion to the whole of the liquid, than in the Newcaſtle coal, amounting to one half of the weight of the liquid. This difference makes me ſuſpect that I committed ſome error, in eſtimating the quantity of oil obtained from the diſtillation of Newcaſtle coal. It is probable, however, that the quantity of oil ſeparable from the ſame kind of coal, may be influenced, in ſome degree, by the manner of performing the operation; and there is, moreover, ſome reaſon to believe, that in different kinds of coal the quantities of oil may be very various; as we obſerve them to differ from each other both in their proneneſs to catch fire, and in their ability to ſupport it: though it muſt be owned, that both theſe effects may as conſiſtently be explained, from the difference of the manner in which the oil may be combined with the other principles of the coal, as from the difference of its quantity.

The correſpondence, with reſpect to the quantity of liquid obtained by the ſeveral diſtillations of Engliſh, German and French pit-coal, would induce one to ſuppoſe that the experiments had been reſpectively made with great accuracy; but the great diſparity, as to the quantity of the reſidues remaining after the extraction of the liquid, ſeems to indicate the contrary; for it is greater, I apprehend, than can be accounted for from the different natures of the coals. The German and French analyſes corroborate each other, and the reader may thence reaſonably be diſpoſed to queſtion the truth of mine. He will permit me, however, to produce a teſtimony in its ſupport, and which is, perhaps, the more to be depended on, as I was not aware of it when the analyſis was made.

Dr. Hales*, by diſtilling 158 grains of Newcaſtle coal, obtained 51 grains of *air*. From 96 ounces he would, according to the ſame proportion, have obtained 30 ounces of air; now the difference between 28 ounces of ſome ſort of matter or other which was loſt during my diſtillation of 96 ounces of coal, and 30 ounces of air, which might have been obtained from the ſame quantity of coal by Dr. Hales' method, is ſo ſmall, that it may eaſily be accounted for from the different modes of operating, or from a ſlight difference in the qualities of the coals. Many naturaliſts have thought that pit-coal has been

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* Vegetab. Stat.

produced from vegetables, and eſpecially from foreſts of reſinous woods, which have been buried in the bowels of the earth; without entering into all the reaſons on which this opinion is founded, I will lay before the reader an account of the products obtainable from different woods by diſtillation.

The woods which I diſtilled were heart of *Oak*, *Box*, *Mahogany*, and *Sallow*. They were all dry and old, and were cut into ſmall pieces, about half an inch ſquare, before they were put into the earthen retort. The annexed table ſhews, at one view, the reſult of the reſpective diſtillations.

	Heart of Oak.	Box.	Mah.	Sall.
Weight diſtilled,	96 oz.	96	96	96
Liquid - -	37¼	61½	33½	48
Reſiduum - -	30	26½	27½	20½
Loſs of weight -	28¾	8	35	27½
	96	96	96	96

The liquids ſeparable from woods by diſtillation, conſiſt partly of oil, but principally of water impregnated with an acid, from which alſo a volatile alkali may be diſengaged. The oil varies in quantity according to the nature of the wood, the hardeſt woods, and the hardeſt parts of the ſame wood abounding moſt in oil; thus both oak and box contained, in the ſame weight of the wood, more oil than ſallow did. That part of the oil which is firſt ſeparated from the wood is ſo light as to float upon water, and of a yellowiſh colour; its quantity, however, is but ſmall, compared with that of the thick, black, heavy oil, which follows it. It appears from this account, that the quality of the liquid ſeparable from wood by diſtillation, is wholly the ſame with that of the liquid ſeparable from pit-coal by the ſame means.

The matter which is loſt during the diſtillation of both wood and pit-coal has been called *air*, and it certainly has one, at leaſt, of the moſt diſtinguiſhing properties of *air*—*permanent elaſticity*, for bladders may be inflated with it as with common air. It does not begin to be ſeparated from either wood or coal till the lighter of the two oils begins to appear; it then ruſhes out with great violence, and unleſs a proper vent be given to it, the ſtrongeſt veſſels will be burſt by it.

In diſtilling heart of oak, during the courſe of chemical lectures which I read in the univerſity of Cambridge, in the year 1767, I remarked to the audience, that the air which iſſued with great violence from the oak was inflammable, not only at its firſt exit from the diſtilling veſſel, but after it had been made to paſs through two high bended glaſs tubes, and three large veſſels of water. I at firſt ſuppoſed the inflammability to proceed from ſome oily particles which had accompanied the air, and which I thought would ſoon be condenſed by the could, though they had not been condenſed in paſſing through the water; but upon repeating the experiment, and collecting the air in bladders, I found that it retained its inflammability a long time, and burned like the air, ſeparable from ſome metals by ſolution in acids.

From the reſearches which have been lately made on this ſubject, it is now believed, that this kind of air may be ſeparated from all vegetable, animal, and inflammable mineral ſubſtances by diſtillation; and the common burning of wood, coal, paper, pitch, oil, and other combuſtible bodies, is attributed to this air, which being ſeparated from the body by the heat, and inflamed by the contact of the fire, continues to burn as long as the body continues to afford a proper ſupply of it. *Inflammable air*, conſidered in this view, bears a great reſemblance to what the chemiſts have hitherto underſtood by their *phlogiſton*, or food of fire.

Diſtillation is not the only means of ſeparating *inflammable air*, from vegetable and animal ſubſtances, it is found to be produced by *putrefaction*. Stagnant waters, whoſe bottoms are covered with putrefying vegetables, yield an inflammable air; the *Thames* water, in becoming putrid, yields an inflammable air: and the ſame obſervation may, probably, be true of other river waters which are ſubject to putrefaction. An inflammable air has been obſerved alſo in ſwampy grounds, in cellars, and in privies. There are many inſtances recorded, of a vapour iſſuing from the ſtomachs of dead perſons, which took fire on the approach of a candle*; the matter producing that vapour could not, probably, become putrid enough in the ſtomach, to generate an inflammable vapour, whilſt the perſons were alive; for Van Helmont has remarked, that the air diſcharged from the ſtomach by eructation, is not inflammable, but that it becomes ſo in the lower inteſtines†.

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* Collect. Acad. Tom. III. Part Etrang. p. 16.

† Ructus ſive flatus originalis in *ſtomacho*, prout et flatus Ilei extinguant flammam candelæ. *Stercoreus* autem flatus, qui in ultimis formatur inteſtinis, atque per anum erumpit, tranſmiſſus, per flammam candelæ tranſvolando accenditur, ac flammam diverſicolorem, iridis inſtar exprimit. Van Helmont, Oper. p. 405.

From heart of oak moderately dried, Dr. Hales got one third of the weight of the oak of the air here ſpoken of; this proportion ſufficiently agrees with the quantity which was loſt during the forementioned diſtillation of oak.

A cubic foot of dry *box-wood* weighs, according to Mr. Cotes' table of ſpecific gravities, 1030 ounces; hence, ſuppoſing that 96 ounces of ſuch wood had only given 61 ounces of liquid, it may appear by an eaſy calculation, that a cubic foot of dry box-wood contains above 5 gallons of liquid; ſuppoſing the liquid to be no heavier than water; and if we admit the oil and the acid, when concentered, to amount to two quarts, the other 18 quarts may be looked upon as pure water; nay, the quantity

tity of water may be conſidered as amounting to almoſt the whole of the liquid, ſince both acids and oils principally conſiſt of water.

A cubic foot of *ſallow* weighs, according to Muſchenbroek, 585 ounces, and ſuppoſing that 96 ounces of ſuch wood would yield by diſtillation 48 ounces of liquid, then a cubic foot would contain about 9 quarts of liquid, which is not half ſo much as an equal bulk of box-wood contains.

I put the matter in theſe different lights, in order to excite thoſe who have leiſure to make ſimilar inquiries, for it is undoubtedly a very wonderful thing, that ſuch large quantities of *water*, and *air*, ſhould be combined with the *earth* of wood, by ſuch a peculiar bond of union as

to remain for ages without loſing any of their diſtinguiſhing properties. And here I am ſenſible is great room for many phyſical inveſtigations. — Are the denſeſt woods the moſt abundant in air and water? Is the air *generated* by the proceſs of diſtillation, or only *diſengaged* from the wood, into the compoſition of which it had entered, together with water, by *vegetation?* Have the *inflammable airs*, ſeparable from different woods by diſtillation, the ſame ſpecific gravity and the ſame degree of inflammability? Or is the inflammability influenced by the nature of the *oil* contained in the wood? How long will a given quantity of air retain its inflammability? Does it loſe it in conſequence of the *precipitation of ſome oily particles*, which are diſ-
ſolved

ſolved in it, when it is firſt diſtilled? As no particle of matter can be annihilated, what becomes of the inflammable air, after it has been inflamed? Is it reduced to an unelaſtic *earth*, or does it ſtill exiſt under the form of an uninflammable elaſtic *fluid?* Whence is it that no air begins to be diſtilled, till the moſt volatile part of the oil begins to be ſeparated from the wood? *Fraxinella* is a very odorous plant; when in full bloſſom, the air which ſurrounds it in a ſtill night, may be inflamed by the approach of a lighted candle: Does this inflammability proceed from an inflammable air, which is exhaled by the plant, or from ſome of the finer particles of the *oil* of the plant, being *diſſolved* in the common air of the atmoſphere? Whence is it that the oils diſtilled from wood, during the ſame operation, are of different colours, conſiſtencies, and ſpecific gravities? Does this variety depend upon the loſs of the air, or upon the quantity of the acid mixed with the oils? Are the oils and acids of different woods diſtinguiſhable from one another, by any ſpecific differences, or do they only differ from one another as to more or leſs? Suppoſing the acids obtained from different woods to be of the ſame ſort, and to be purified to the greateſt degree, what kind of ſalts would they form, when united with alkalies, earths, and metallic ſubſtances? In what manner are the relative proportions of the water, acid, oils, air and reſiduum ſeparable from wood by diſtillation, influenced by the operation being conducted with a quick or a ſlow fire?

Theſe, and a great many ſimilar queſtions, might be reſolved with the greateſt accuracy, and our knowledge be thereby much advanced. But it falls to the lot of few to be able to build commodious Elaboratories, to purchaſe ſuitable inſtruments, to ſupply proper materials, and to maintain operators to execute the infinite variety of experiments which might be deviſed, for the regular and ſyſtematic improvement of every branch of Chemiſtry.

The reſidue remaining in the retort, after the diſtillation of the wood, is a perfect charcoal, and that which remains after the diſtillation of pit-coal, is a light ſpungy maſs, in appearance,

pearance, and, indeed, in quality, reſembling a ſubſtance prepared from pit-coal, as an article of trade, and which is uſually called *coak* or *cinder*.

At *Newcaſtle* 24 barrows of coals produced generally but 18 barrows of cinders, and the price of a buſhel, or of any other meaſure of cinders, is commonly one third more than the price of the ſame meaſure of coals *. At *Cambridge*, on the contrary, where great quantities of cinders are prepared for the drying of malt in the town and neighbouring villages, the price of cinders and of coals is the ſame. From this circumſtance it might probably be inferred, that the coal was increaſed in bulk by being converted into cinder, otherwiſe the Merchant would have no compenſa-

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* Voy. Metal. par M. Jars. p. 211.

tion for the expence attending the proceſs. Upon examining the fact, I found that in one inſtance 30 buſhels of coal gave 39 buſhels of cinders, and in another 28 buſhels of coal gave 36 buſhels of cinders. The quantity of cinders, being eſtimated by *meaſure*, depends much upon the cinders being in rounder lumps than the coals; and the real quantity, eſtimated by *weight*, depends greatly upon the degree of heat to which the cinder has been expoſed. I take the difference in the bulks of the Newcaſtle and Cambridge cinder, procurable from an equal quantity of coals, to proceed from the different degrees of heat to which they are expoſed; the Cambridge coak ovens being drawn once in 24 hours, and thoſe at Newcaſtle only once in 48 hours, or thereabouts.

In order to ſee what effect a longer continuation of fire would have upon the ordinary Cambridge cinder, I took a large piece, and having broken it, threw one part of it into the middle of a fierce cinder fire, employed in the drying of malt. This part became red hot in leſs than ten minutes; after it had continued in that ſituation for above two hours, I took it out, and when it was cold I found it conſiderably waſted in bulk, but it appeared to be harder and blacker than the other part of the ſame piece, and its parts were certainly more condenſed; for I found that a cubic foot of the ordinary cinder weighed 1090 ounces, and a cubic foot of that which had been expoſed to the fire for two hours, weighed 1208 ounces. This cinder was, probably, as much changed in the ſpace of two hours, by the ſtrong draught of the fire in which it was put, as it would have been by a much longer continuance in the coak oven, where the draught of air is very little: and from this experiment it may be inferred, that the quantity of cinders procurable from a definite quantity of coals, depends very much upon the ſtrength and continuance of the fire, ſince the cinder, after it is fully formed, is not only waſted in part by a longer continuance in the oven, but the part which remains is rendered more denſe, and upon both accounts the quantity, eſtimated by meaſure, is leſſened. This obſervation may be of uſe to thoſe who are employed in preparing cinder for ſale; and upon inquiry I find, that the quantity of cinder procured from the ſame quantity of coal is very variable: when the operation has been longer, or the heat greater than uſual, inſtead of gaining of ſix or eight buſhels from every thirty buſhels of coal, they ſometimes are not able to get even 30 buſhels of cinder.

The conſumption of charcoal in fluxing iron from its ores, and in manufacturing it into bar iron, is ſo very conſiderable, and the price of charcoal, from the great ſcarcity of under-wood in this kingdom, is ſo great, that many attempts have been made to ſubſtitute in this buſineſs charred pit-coal in the room of charcoal. Theſe attempts have in part ſucceeded, and iron is now very generally extracted from its ore by fires made with coak. But the iron thus procured is very fragile and coarſe, and cannot, without great difficulty, be rendered as malleable as that which is fluxed with charcoal; it may anſwer nearly as well for caſting, but it cannot be made into good bar iron by any art hitherto known. Hence it became an object worthy the contemplation of the ſociety for the encouragement of arts, manufactures, and commerce, to propoſe premiums for the making of pig iron, and converting pig iron into bar iron with coak. The pigs and bars were required to be as good as thoſe made with charcoal, but the premiums, though propoſed from 1762 to 1766 were never claimed*.

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* Memoirs of Agriculture, Vol. I. p. 220

Thoſe who are intereſted in iron works, have motives, more prevalent than the ſociety's premiums, to induce them to perfect the art of ſmelting by means of charred pit-coal; and it may not be an uſeleſs hint to them, to try the effect of coak, which has undergone different degrees of heat. I ordered a ton of coals to be put into a coak oven, and when it was in the uſual way converted into a cinder, I found that it weighed 11 hundred weight; had it ſtayed longer in the oven it would have been reduced further, and it may deſerve to be inquired, whether there is not a definite weight to which any given quantity of coals ought to be reduced, in order to produce the beſt poſſible coak for the fluxing of iron ore. The * *Alais* pit-coal is ſaid to loſe half of its weight by being reduced to coak, whilſt another ſort of coal loſt only 35 parts in an hundred: this diverſity may depend partly on the nature of the coal, but principally, I conceive, on the degree and continuance of the fire uſed in the preparation of the coak.

" What ſhall I ſay further of coals, except I tell you of one *Becher*, in King Charles the Second's time, who pretended to make *pitch* and *tar* from them, but I think his project came to nothing†." This is *Houghton*'s obſervation concerning a project of one of the greateſt chemiſts that ever lived. Whatever fate his project met with in his own age, it has been realized in ours; for a kind of tar has for ſeveral years laſt paſt been prepared from coal in the biſhopric of *Liege*, and in other parts of Germany; we alſo make conſiderable quantities in England, eſpecially near *Broſeley*, and at *Briſtol* *; a perſon at Briſtol having obtained a patent for making tar from pit-coal, the condition of which, if I am rightly informed, is, that every perſon who undertakes to procure tar from pit-coal muſt pay a ſhilling a barrel to the patentee.

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* Inſtruc. ſur l'Uſage de la Houille, p. 81.

† Houghton's Collec. Vol. II. No. 242.

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* In the Philoſ. Tranſ. No. 228, year 1697, there is mention made of pitch and tar being then ſeparated at *Broſeley*, in *Shropſhire*, from the black bituminous ſtone which lies over the coal. It was ſeparated, partly by boiling the ſtone, and partly by diſtilling it—the preſent method of obtaining pitch from pit-coal is a great improvement upon the former practice.

The method of making *tar* is thus deſcribed by Newmann.—“ Tar is prepared in different parts of Germany, Norway, Sweden, &c. from the pine and the fir-trees, and in ſome places from the larch and the terebinth. The wood is incloſed in a large oven to the quantity of ten or more loads at a time: this ſtands within another oven called the mantle, the ſpace between them receiving the fire. From the bottom of the inner oven runs a gutter, by which the tar is conveyed off in proportion as it melts out of the wood *.” This proceſs is a kind of diſtillation, in which the inner oven repreſents the body of the retort, and the gutter its neck. The fluid parts of reſinous woods are probably capable of being melted out of them with a leſs degree of heat than what is requiſite for the diſtillation of coal, yet the proceſs for procuring tar, here mentioned, might be applied, with a few alterations, to the obtaining the thick oil of coals.—There would in this plan be no loſs of fuel, the coal contained in the inner oven would be converted into one of the moſt valuable kinds of coak, and the fire between the ovens would afford a cinder as good as the common cinder.

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* Newmann's Chem. p. 288.

In the biſhopric of *Liege* the coal is diſtilled in a kind of ſtill compoſed of two large caſt-iron pots. In England the coal is put into ovens which are heated by fires lighted under their bottoms, and the liquid matter is forced through an iron pipe inſerted into the top of the oven, and which communicates with proper condenſing veſſels. As the pitchy oil is very heavy, the diſtillation, I think, would be moſt advantageouſly performed by making the fire on the top of the oven containing the coal, ſo that the liquid matter might be made to deſcend through a pipe inſerted into the bottom of the oven, and communicating with proper receivers.

In the great works they obtain the very ſame ſubſtances which we got from the analyſis of coal—a corroſive watery liquor, and two ſorts of oil. The two oils are put into an iron ſtill, in order to their being further purified from the watery liquor with which they are mixed; 6 barrels of this oily matter produce about 5 barrels of oil of a thicker conſiſtence; of the oil thus thickened one part is lighter than the other, the lighter part is drawn off from the other, and is not at preſent applied to any uſe; the thicker part is uſed as *tar*, for the paying the ſides and bottoms of ſhips, but it is not found to anſwer ſo well as tar, either for cordage, or to mix with oakum.

Thoſe who are intereſted in the preparation of coak would do well to remember, that every 96 ounces of coal would furniſh 4 ounces at the leaſt of oil, probably 6 ounces might be obtained; but if we put the product ſo low as 5 ounces from 100, and ſuppoſe a coak oven to work off only 100 tons of coal in a year, there would be a ſaving of 5 tons of oil, which would yield above 4 tons of tar: the requiſite alteration in the ſtructure of the coak ovens, ſo as to make them a kind of diſtilling veſſels, might be made at a very trifling expence.

It has been before obſerved, that wood and pit-coal wholly reſemble each other in the products which they yield by diſtillation, and as the oil from pit-coal is found to anſwer the purpoſe of tar, it might ſurely be a matter worthy the conſideration of thoſe who are engaged in the burning of wood into *charcoal*, to contrive means which might eaſily be done, of ſaving all the oil which is ſeparated from the wood during that operation.

The acid liquor which is procured from pit-coal, when it is diſtilled for tar, is at preſent thrown away, as I have been informed by a perſon who is much engaged in this buſineſs. Without entering into the diſpute concerning the origin of the vegetable fixed alkali, we may obſerve, that it is generally obtained from the combuſtion of the vegetable, and there is reaſon to think, that the *acid* of the vegetable contributes in ſome way or other towards its production: thus the concrete acid of tartar yields, by being burned, a great quantity of fixed alkali. Is it wholly improbable, that the *acid* liquor which is ſeparated from the coal by diſtillation, might be converted into a *fixed alkali* by being boiled down and calcined, either by itſelf, or in conjunction with ſtraw, charcoal duſt, or any other ſubſtance, containing the inflammable principle?

The making of fixed alkali in Great Britain, is a matter of ſuch great national concern, that the commercial reader will excuſe me, if I give another hint or two upon the ſubject. Might not pot-aſh be prepared in the cyder countries, from the calcination of the *acid* earthy pulp, which remains after the juice is preſſed out from the apple? The brewery *grains* are at preſent applied to ſome uſe, would they not, if ſuffered to turn *ſour*, yield more profit by being calcined for pot-aſh? The brewery hops, which are impregnated with the ſweet wort, would probably turn ſour, and as they are thrown away as uſeleſs, might it not be adviſable to ſee whether they would not yield pot-aſh by calcination? The lees and refuſe from the making of vinegar, might be tried for the ſame purpoſe; and ſo might all the water which is impregnated with the waſhings of the floors, veſſels, &c. in which any vegetable liquor is fermented, either into ale or vinegar. I remember having evaporated, ſeveral years ago, a large quantity of horſe's urine, with a view of ſeeing whether it would yield by cryſtallization, the ſame kind of *phoſphoric* ſalt, which may be obtained from human urine; I could not procure any ſalt of that kind from it; but when it was evaporated to dryneſs, it yielded, even without *calcination*, a great deal of *fixed alkali*. The urine of animals, which feed upon vegetables, ariſes from the juices of the vegetables, which have been changed in ſome degree by the organs of digeſtion; may not a ſimilar change take place by putrefaction? and may not the putrefaction of vegetable juices be increaſed, by a ſmall admixture of common ſalt? and would not the quantity of alkali be thereby increaſed? Is it not poſſible to *decompoſe* common ſalt, by mixing it with vegetables in a ſtate of putrefaction? If the acid of ſalt could by any cheap means be ſeparated from its alkaline baſis, it would undoubtedly be the intereſt of government to exempt from the ſalt duty, all ſuch rock or ſea ſalt, as ſhould be converted into a fixed alkali; for by that means, all the money might be kept in the kingdom, which is now ſent out of it, for the purchaſe of barilla, and other ſorts of fixed alkali.

It has, with ſome men, been a ſubject of apprehenſion, leſt all the coal-pits in England ſhould ſoon become exhauſted. The quantity of coal which is annually raiſed, is certainly very great; and as coal is not found to grow again, the time muſt come, when it will all be conſumed. That æra, however, is certainly at a great diſtance, ſince in thoſe counties where coal is found, they are conſtantly opening freſh pits, and may for centuries to come continue to open more; and coal may, probably, be diſcovered in many counties, where none is extracted at preſent.

ſent. It will enable the reader to form ſome notion of the great excavations, which are annually made in this iſland from the digging of coal, if we calculate the extent of that which is made in one year, in conſequence of the coal which is imported into London. The following account may be relied on:

An account of coals imported into the port of London, in ten years, ending at Chriſtmas, 1779.

Chriſtmas	1770	615,330 chalders.
	1771	694,003
	1772	725,008
	1773	624,781
	1774	623,727
	1775	672,786
	1776	700,207
	1777	697,435
	1778	647,361
	1779	587,895
Annual average		658,853

No accounts are kept at the Cuſtom Houſe, diſtinguiſhing the importation from Newcaſtle, Sunderland, Blythe, Hartlepool, &c. or from the ſeveral parts in Scotland and Wales, the Crown revenue not being affected thereby. The duty is 8 s. per chalder of 36 Wincheſter buſhels, on all coals ſold by meaſure, and 6 s. 4 d. per ton, on ſuch as are ſold by weight. It appears from the preceding account, that in the year 1777, there were imported into the port of London 697,435 chalders, and we learn from another authority*, that in the ſame year there were imported from *Newcaſtle* and *Sunderland* alone 692,093$\frac{3}{4}$ chalders; by comparing theſe numbers, we may obſerve how ſmall a portion of the

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* Ann. Reg. 1777, Ch. 161.

the coal conſumed in London, is brought from any other quarter.

A chalder of coals, Newcaſtle meaſure, is allowed by Act of Parliament * to weigh 53 hundred weight: and as 8 Newcaſtle chalders are equal to 15 London chalders, reckoning 36 Wincheſter buſhels to the London chalder, it follows, that a chalder of coals, London meaſure, is equal to a little more than 28 hundred weight. But ſuppoſing 28 hundred weight to be the weight of a London chalder, then as the medium quantity of coal annually imported into London is 658,853 chalders, it may be collected, that there are annually imported 922,394 tons.

The following table expreſſes the relative

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* 6 & 7 W. & M. III. c. 10. ſ. 2.

relative weights of a few different ſorts of coals.

Weight of a cubic foot of

Welſh coal - - -	1396	ounces avoir.
Yorkſhire ſtone-coal	1307	——
Lancaſh. cannel-coal	1273	——
Newcaſtle upon Tyne	1271	——
* Staffordſhire coal	1241	——
‡ Coal de Graiſſeſac	1371	——
‡ Coal des Carmaux	1333	——
‡ Coal d' Alais - -	1500	——
Medium weight	1336	ounces avoir.

The Welſh coal was of that kind which burns without giving any ſmoke, they mix it here with cinder for the drying of malt. The Yorkſhire ſtone-coal was taken from a pit near

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* Philoſ. Tranſ. No. 169.

‡ The weights of the three French coals were calculated from experiments mentioned in Inſtruct. ſur l'Uſage de Houille, p. 2.

near Wakefield, and is part of the ſtratum, which lies at the bottom of the black coal. The Cannel coal was from Wigan in Lancaſhire: this coal is commonly uſed by the country people to work by in winter nights, inſtead of candles, and its name has been probably derived from its uſe, candles being called in the dialect of the northern counties, cannels.

The Newcaſtle coal was of a kind which is much eſteemed at Cambridge, and which is called *Old Ducks*. The medium weight of a cubic foot of theſe ſeveral coals being 1336 avoirdupois ounces, a cubic yard or 27 cubic feet of coal, will weigh 2254½ avoirdupois pounds; there are 2240 ſuch pounds in a ton, hence we may ſay in general, that a cubic yard of pit-coal weighs a ton; ſome ſorts of coal weigh a few pounds more, and other, a few pounds leſs than a ton. From what has been before obſerved concerning the quantity of coal annually imported into London, we may now conclude that 922,394 cubic yards of coal are annually dug out of the earth for the ſupply of the London market. This quantity of coal, if ſpread to the thickneſs of an inch, would cover an area of above 10 ſquare miles; and no doubt, the conſumption of ſo large a quantity contributes very much both to the daily dirtineſs of the ſtreets of London, and to that elevation of ſoil which is ſufficiently obvious in the city and its environs, when diſtant periods of time are compared together.

The weight of a London chalder of coals is here put equal to 28 hundred weight, upon the ſuppoſition of 8 Newcaſtle chalders being equal to 15 London chalders. Upon weighing a buſhel of the middle ſized, large, and ſmall *old ducks*, at Cambridge, I found the weights to be

Middle ſized -	74
Large coal - -	76
Small coal - -	80
Medium -	$76\frac{2}{3}$

This medium does not give quite 25 hundred weight to a chalder. I found, by weighing coals at another warehouſe in Cambridge, that 30 buſhels made a ton, which is $74\frac{2}{3}$ pounds to a buſhel.

It is not certainly known how long coal has been dug in Great

Britain; the firſt ſort of fuel which all nations uſed was probably wood, and as that grew ſcarce men began to ſearch into the bowels of the earth for ſomething to ſupply its place. We have good reaſon to believe that the Newcaſtle coal pits were wrought in the time of the Romans, for coal cinders have been found at the bottom of the foundation of a city built by the Romans in that county*; but whether they were not wrought by the Britons before the Roman invaſion, is a queſtion which cannot, perhaps, be poſitively decided either way.

Anderſon, in his Hiſtory of Commerce, fixes the introduction of Newcaſtle coal into London at ſo early a period as the year 1305; and mentions a complaint of the nobility and gentry, which was preferred to the King againſt the uſe of ſea-coal, as a public nuiſance. A ſimilar prejudice prevails at this day, among thoſe who have not been accuſtomed to pit-coal fires. The great Hoffman had given it as his opinion, that the peripneumony, the dry aſthma, and the conſumption were common maladies amongſt the inhabitants of Liege and London, and that they were occaſioned by the great uſe of pit-coal in thoſe places. M. Morand, in 1769, was at the trouble to inquire into this matter, and it appears from the atteſtations of the College of Phyſicians in *London*; of the Faculty of Medicine at *Paris*; of the phyſicians at *Liege*, at *Lyons*, and other places, that the opinion concerning the peculiar inſalubrity of pit-coal fires is utterly without foundation.

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* Wallis's Hiſt. of Northumberland.

It has been remarked that coal, in being diſtilled, yields near $\frac{1}{3}$ of its weight of air; in being conſumed by burning, it yields a great deal more; for both the reſidue remaining in the retort, and the oil obtained by diſtillation, yield much air during combuſtion. I do not think that it would be an extravagant ſuppoſition, if we ſhould ſay, that the 922,394 tons of coal which are annually conſumed, diſperſed into the atmoſphere incumbent over London, 500000 tons of air; and this quantity may, probably, be about $\frac{1}{4}$ of the whole quantity of air, which is reſpired by the inhabitants of London in the courſe of a year.

Hence, though it ſhould be granted that the air from coal is not more unwholeſome than that which is ſeparated from any other kind of fuel, yet the vaſt quantity of it with which the common air over great cities is daily infected, may contribute ſomething to the rendering them, what they have been juſtly called, the GRAVES of mankind.

END OF VOL. II.

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